SCIENCE

6 June 1958

Volume 127, Number 3310

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PERIODICAL READING ROOM

Editorial Nuclear Ships 1315

Articles High Atmosphere Densities: *M. Nicolet* 1317

To explain satellite observations, models of the thermosphere must allow for diffusion and other factors.

New Approach to Teaching Intermediate Mathematics: *K. Menger* 1320

It is based on a resolution of the spectra of meanings of the letter *x* and the term *variable*.

A Human Enterprise: *H. K. Schilling* 1324

Science as lived by its practitioners bears but little resemblance to science as described in print.

Tatuo Aida, Geneticist: *T. Komai* 1327

News of Science Sputnik III; other recent events 1328

Book Reviews *Russia, the Atom and the West*; other books 1332

Reports Immunochemical Studies with Tomato Leaf Proteins: *R. Rohringer* and *M. A. Stahmann* 1336

Renal Lesions Produced by Group A, Type 12, Streptococci: *D. K. Kelly* and *J. F. Winn* 1337

Red Cell Glucose-6-Phosphate and 6-Phosphogluconic Dehydrogenases and Nucleoside Phosphorylase: *P. A. Marks* 1338

Three Chromosome Numbers in Whites and Japanese: *M. Kodani* 1339

Continuous Measurement of the Depth of Sleep: *D. Lester* 1340

Effect of Chloramphenicol on Glucose Oxidation in *Escherichia coli*: *S. H. G. Allen, Jr.*, and *D. M. Powelson* 1341

Quantitative Histochemistry of the Nephron: *S. L. Bonting* et al. 1342

Preliminary Note on Kimzeyite, a New Zirconium Garnet: *C. Milton* and *L. V. Blade* 1343

Departments Meetings; Letters; Equipment 1344

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

Hermann von Helmholtz...on immortality

"...what arouses our moral feeling at the thought of a future cessation of all living creation on earth, remote as this may be, is above all the question whether all life is but an aimless sport, which will ultimately fall prey to destruction by brute force. In the light of Darwin's great thoughts we begin to see that not only pleasure and joy, but also pain, struggle, and death, are the powerful means by which nature has built up her finer and more perfect forms of life. And we men know that in our intelligence, our civic order, and our morality we are living on the inheritance which our forefathers gathered for us through labor, struggle, and sacrifice; we also know that what we acquire will in like manner ennoble the lives of our descendants. Thus the individual, who works for the ideals of humanity, even if in a modest position and in a limited sphere of activity, can bear without fear the thought that the thread of his own consciousness will one day break."

—*Über die Entstehung des Planetensystems*, 187

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6 June 1958, Volume 127, Number 3310

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Nuclear Ships

The age-old dependence of man upon wind and muscle to propel his ships began to come to an end nearly a century and a half ago when the *Savannah*, equipped with sails and an auxiliary 90-horsepower, one-cylinder steam engine and fueled by 75 tons of coal and 25 cords of wood, became the first American ship to use steam for an Atlantic crossing.

This notable accomplishment is celebrated yearly on National Maritime Day, 22 May. But the celebration this year was more than a commemoration. It was marked by an event that foreshadows another great change in marine transportation: the laying of the keel of the world's first nuclear-powered passenger-cargo vessel, which will be appropriately named the *N. S. Savannah* ("*N.S.*" for *nuclear ship*), after its pioneering predecessor.

The new *Savannah*, which is scheduled to be completed in 1960, will not be outstanding among modern vessels either in power or in speed: its shaft horsepower will be only 22,000, its running speed slightly more than 20 knots. Its significance lies rather in its use as an experimental commercial ship for the determination of the costs and the problems of nuclear propulsion.

So far as costs are concerned, no one expects the *Savannah* to be able to compete with conventional ships: the high initial expense of building—estimated at $40.25 million—will not be offset by its infrequent need for refueling. The *Savannah* is expected to run for some 300,000 miles or about three and one-half years on a single charge of 60 kilograms of enriched uranium oxide.

But in time costs will doubtless be reduced to the point at which nuclear ships will become competitive and more than competitive. Before that time comes, it is well to consider some of the special risks that are inherent in nuclear ships both for their crews and ports of call.

As P. T. Fletcher of the United Kingdom Atomic Energy Authority has recently pointed out [*Atom* **19**, 10 (May 1958)], one of the major problems of using a nuclear reactor in a ship will be to contain the fission products, especially in case of accident. Fire, collision, failure of the cooling system, or corrosion of the containing jacket could bring about release of the radioactive fission products with consequent danger to the crew and passengers if the ship were at sea or to a much larger number of people if the ship were in port.

The hazards can, as Mr. Fletcher notes, be reduced, but not eliminated, by engineering safeguards—the reactors can, for example, be designed for automatic flooding in emergencies—and by specially trained crews alert to all of the possibilities of mishap. In most emergencies the hazards to large numbers could be reduced by towing the ship out to sea.

If nuclear-powered ships are to become useful commercially, the governments of the ports that receive them will be obliged to undertake the new and demanding tasks of monitoring for radioactivity and warning the public in possible emergencies.

Cities that serve as ports will probably be the first to cope with the new problems of the atomic age, but as land-based nuclear reactors become more abundant and as the transportation of uranium fuel and radioactive wastes increases, inland cities will also need to make a suitable response. Perhaps "radiation departments" will become as standard in municipal governments as police and fire departments are now.—G.DuS.

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High Atmosphere Densities

To explain satellite observations, models of the thermosphere must allow for diffusion and other factors.

Marcel Nicolet

Although the densities of the high atmosphere which are extrapolated from rocket data usually lead to low values at altitudes above 300 km, recent values obtained from observations of satellites by Harris and Jastrow (*1*) and by Paetzold (*2*) are best explained by assuming that the densities are relatively high—greater than 10^{-15} g/cm^3 at 500 km.

For example, Kallmann, White, and Newell (*3*) derived a value of 4.4×10^{-15} g/cm^3 at 300 km, and more recently, Miller (*4*) obtained only 2.7×10^{-16} g/cm^3 at 500 km. Because of the current impossibility of finding an atmospheric model based on the usual concept which can explain the satellite observations and even certain ionospheric properties, it is thought that the upper thermosphere has a very high temperature; only thus can the escape time of helium (of the order of 2 million years) be explained as reported by Nicolet (*5*).

This article shows how the concept of the thermosphere must be considered according to the view of Chapman (*6*) on the extension of the solar corona toward the earth's atmosphere and according to the atmospheric structure determined by Nicolet (*7*), who introduced diffusion distributions for all constituents at altitudes above 110 km.

Dr. Nicolet is general secretary of the Special Committee for the International Geophysical Year and head of the radiation department at the Royal Meteorological Institute of Belgium, at Uccle, Brussels.

The Thermosphere

The region above the deep minimum of temperature, near 85 km, corresponds to the upper atmosphere, in which the temperature increases with height. If the temperature distribution depends only on the rate of absorption of the energy emitted by the sun in the ultraviolet, it is not possible to have any increase in temperature above 300 km. For this reason, the atmospheric model adopted by Johnson (*8*) is limited to a temperature maximum of about 1100°K at 300 km. Furthermore, according to Lowan's (*9*) calculation, this model cannot be applied to night conditions since there is no solar energy available. Besides, Bates (*10*) has shown that it is difficult to maintain a large temperature gradient without very large amounts of energy being absorbed by the atmosphere at 200 km.

All the atmospheric models based on rocket results up to 200 km rest upon an assumption which requires a constant ratio—namely,

$$\frac{n(O) + 2n(O_2)}{n(N) + 2n(N_2)} = 1/4,$$

where n is the concentration—that is to say, they rest on conditions prevailing during perfect mixing such as is found in the lower atmosphere. Such a hypothesis leads to a deficiency of atomic oxygen in the thermosphere if the concentration of molecular oxygen observed up to 180 km by Byram, Chubb, and Friedman (*11*) is taken into account in the computations. However, a restriction such as that imposed by a constant oxygen-nitrogen ratio does not apply to aeronomic conditions in which transport is involved. First, the vertical distribution of atomic and molecular oxygen in the thermosphere above 100 km must be considered to be due to diffusion. [Nicolet and Mange (*12*) have shown that the oxygen distribution departs from photochemical equilibrium conditions]. Second, the production of almost 10^{11} oxygen atoms per square centimeter per second above 100 km modifies the constancy of the oxygen-nitrogen ratio since, as shown by Nicolet (*13*), atomic oxygen must recombine below 100 km after a downward transport from the highest altitudes. Finally, the absolute values of the atomic oxygen concentration should be fixed by the secular equilibrium between the total production of oxygen atoms and their recombination. Comparing the possible concentrations of atomic oxygen between the 100-km and 110-km levels, at which the downward transport and equilibrium conditions prevail, respectively, it is seen that

$$\tfrac{1}{2} n(N_2) \leq n(O) \leq n(N_2);$$

that is to say, the oxygen-nitrogen ratio is greater than the value given by a constant mixing ratio for which $2\, n(O) \leq n(N_2)$.

It may be pointed out that an arbitrary hypothesis has been made to explain rocket data—that is, a low mean molecular mass as low as $M = 22.3$ at 180 km. Such a low mass corresponds to a constant mixing ratio in which dissociation of nitrogen is introduced. However, atomic nitrogen is not present to such an extent that the mean molecular mass is less than the minimum mass $M = 24$. Horowitz and Lagow (*14*), in analyzing the rocket data of densities, adopted, for the region between 100 and 180 km, the constant oxygen-nitrogen ratio used by Johnson (*8*). However, evidence for the dissociation of N_2 does not exist, and atomic nitrogen is only a minor constituent. When atomic nitrogen is produced in the thermosphere, it reacts immedi-

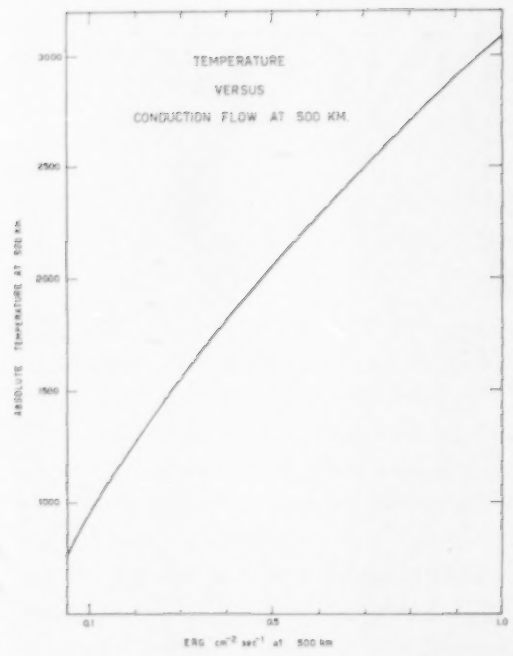

Fig. 1. Flow of energies between 0.1 and 1 erg cm^{-2} sec^{-1} in the atmosphere at an altitude of 500 km, where the principal constituent is atomic oxygen, leads to temperatures between 1000° and 3000°K.

ately, above 110 km, with molecular oxygen according to the process

$$N + O_2 \rightarrow NO + O$$

and disappears by the reaction

$$NO + N \rightarrow N_2 + O.$$

The life-time of a nitrogen atom is less than one day at altitudes below 200 km. Therefore there is insufficient dissociation of nitrogen, and any working model of the atmosphere can incorporate atomic nitrogen only as a minor constituent, while the concentration of nitric oxide is a definite fraction of the concentration of molecular oxygen in the whole atmosphere. A dissociation beginning at 120 km and being complete at 220 km, such as that adopted by the Rocket Panel (*15*), is not acceptable for studying the behavior of the thermosphere. In fact, vertical distributions corresponding to linear increases of the dissociation of oxygen or nitrogen cannot be stable when the diffusion times, as determined by Mange (*16*), are used. Nicolet (*7*), using the criteria of Mange, has shown that times corresponding to a change from a mixing distribution to diffusion can be found. Diffusion of all constituents from the mixing state is complete in about one day when it begins at an altitude of 120 km, but requires at least one week when it begins at an altitude of 100 km. In other words, whatever the mean molecular mass may be, it is not possible to maintain mixing above an altitude of 100 km when the process requires less than one day, and, therefore, diffusion of all constituents must take place between altitudes of 100 and 110 km. In the light of these results, the observational data of Townsend (*17*) on the argon-nitrogen ratio are interpreted by a diffusive separation which may begin at altitudes as low as 110 km.

Consequently, it may be concluded that the atmospheric mean molecular mass varies with altitude in the thermosphere and is associated with the varying ratio of the concentrations of atomic oxygen and molecular nitrogen.

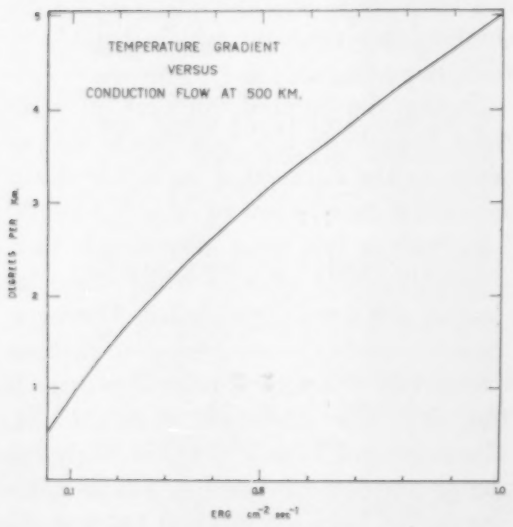

Fig. 2. The gradient of the temperature at 500 km depends on the conduction flow in an atmosphere of atomic oxygen. Gradients of between 1° and 5° per kilometer correspond to energies of between 0.1 and 1 erg cm^{-2} sec^{-1}.

Thermal Conductivity and the Thermosphere

If there is no thermal flux arriving at the top of the atmosphere, the production of heat by absorption of solar radiation does not permit, as was shown by Bates (*10*), very high temperatures above the electron peak of the ionospheric F layer to be inferred, for no ultraviolet radiation absorption process occurs there. The energy supply must be in accordance with Chapman's (*6*) deduction of heat flow by conduction due to the extension of the solar corona. Chapman found that it is possible for 2.4×10^{19} erg/sec to be available to the whole terrestrial atmosphere at a distance of five earth's radii. Thus, if any significant fraction of this energy is trapped, a high temperature above 300 km is possible. Furthermore, Chapman (*18*) has shown that atomic hydrogen must be involved in heat transport from the external corona to the earth's atmosphere, since the thermal conductivity, λ_I, of ionized gas is, according to Chapman (*6*),

$$\lambda_I = 5.2 \times 10^{-7}\, T^{5/2} \text{ erg cm sec } ^\circ\text{K}$$

and that λ_o of neutral atomic oxygen

$$\lambda_o = 3.6 \times 10^{2}\, T^{1/2} \text{ erg cm sec } ^\circ\text{K}$$

it is certain that at 500 km (the atmospheric region that we are considering here), the conductivity corresponds to heat transport in the atomic oxygen gas

Discussion and Results

We may therefore consider that there is an energy flow descending as heat to the upper part of the thermosphere. For example, the temperature distribution in the neighborhood of 500 km is maintained in a steady state in the neutral atomic oxygen gas by downward conduction which removes the heat received from the region surrounding the earth at the same rate as that by which it is introduced. At these altitudes, the heat production by absorption of ultraviolet radiation is certainly negligible, and the heat $R(O)$ radiated by atomic oxygen

$$R(O) = 1.65 \times 10^{-18} n(O) \text{ erg/cm}^3 \text{ sec}$$

can also be neglected.

Consequently, the heat flux F at a distance r from the earth's center being

$$F = 4\pi r^2 A T^{1/2} \frac{dT}{dr}$$

in which $A = 3.6 \times 10^2$ is the coefficient for atomic oxygen, the law of the vertical

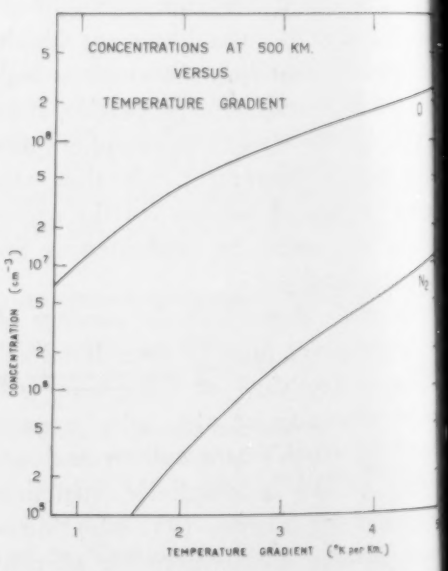

Fig. 3. The number of nitrogen molecules and oxygen atoms depends on the temperature gradient in the thermosphere. The variation is shown for temperature gradients of between 1° and 5° at 500 km

tribution of temperature is given as ows (9):

$$\frac{T_z^{3/2} - T_0^{3/2}}{T_h^{3/2} - T_0^{3/2}} = \frac{z}{h}\,\frac{r_h}{r}$$

ere T_h and T_0 are, respectively, the perature at reference levels $z = h$ and $z = 0$, T_z is the temperature at any level above $z = 0$, and r_h and r are the respective distances from the center of the earth to the specific level h and the general level z.

The two preceding relations lead to the following relation for the rate of conduction of energy E_h (ergs per square centimeter per second) at level h (*10*).

$$E_h = \frac{r_0}{r_h}\,\frac{2}{3}\,A\,\frac{T_h^{3/2} - T_0^{3/2}}{h}$$

If one identifies $h \equiv 500$ km and adopts a rate of conduction of energy in the possible range from 0.1 to 1.0 erg cm^{-2} sec^{-1}, temperature values from 1000°K to 3000°K are derived (see Fig. 1). On this basis, the temperature gradients at 500 km vary between 1° and 5°K per kilometer, as shown in Fig. 2.

By considering specific values for the concentrations of molecular nitrogen and atomic oxygen at level $z = 0$—namely, an altitude of 140 km where the temperature is 560°K—it is possible to obtain the concentrations at 500 km. With $n(O) = 5.60 \times 10^{10}$ cm^{-3} and $n(N_2) = 4.15 \times 10^{10}$ cm^{-3}, the concentrations of oxygen atoms and nitrogen molecules at 500 km are given in Fig. 3. The results are (i) a low concentration of N_2; and (ii) a relatively small variation of atomic oxygen concentration for temperature gradients between 3° and 5°K per kilometer.

If the concentrations so obtained are related to the mass density ρ by the relation $\rho = \Sigma nm$, where m is the atomic or molecular mass, the densities at 500 km are obtained for the temperature range 1000 to 3000°K. It can be seen from Fig. 4 that densities greater than 10^{-15} g/cm^3 require temperatures higher than 1500°K. Therefore, temperatures of not less than 2250°K, inferred by Nicolet (*5*) for the escape of helium, lead to densities not less than 4×10^{-15} g/cm^3 at 500 km.

By assuming a specific value for the heat flow, namely 0.6 erg cm^{-2} sec^{-1}, leading to a temperature of the order of 2250°K and a temperature gradient of 3.5°K per kilometer at 500 km, the variation of density with height may be used to provide an average vertical distribution such as that shown in Fig. 5. The crosses indicate the degree of the effect of temperatures higher or lower than 2250°K on the density at 500 km.

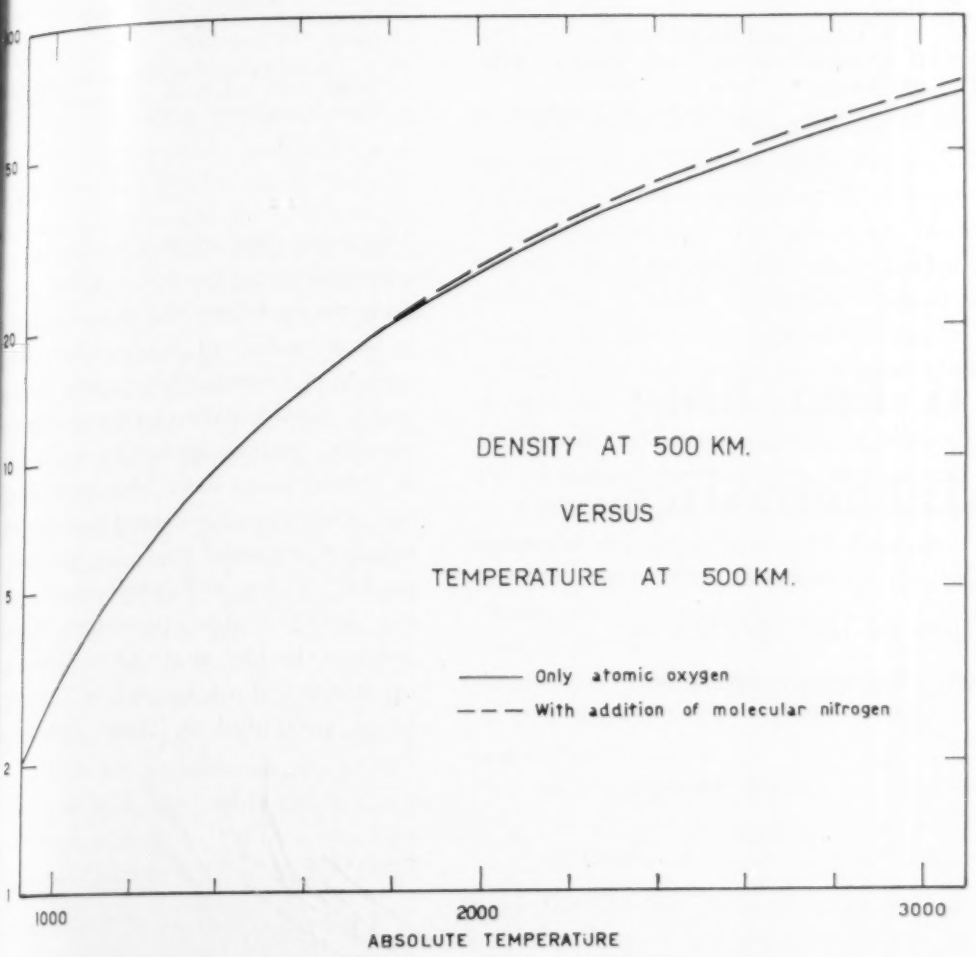

Fig. 4. The density of the atmosphere at 500 km varies by a factor of about 25 when temperatures vary between 1000° and 3000°K.

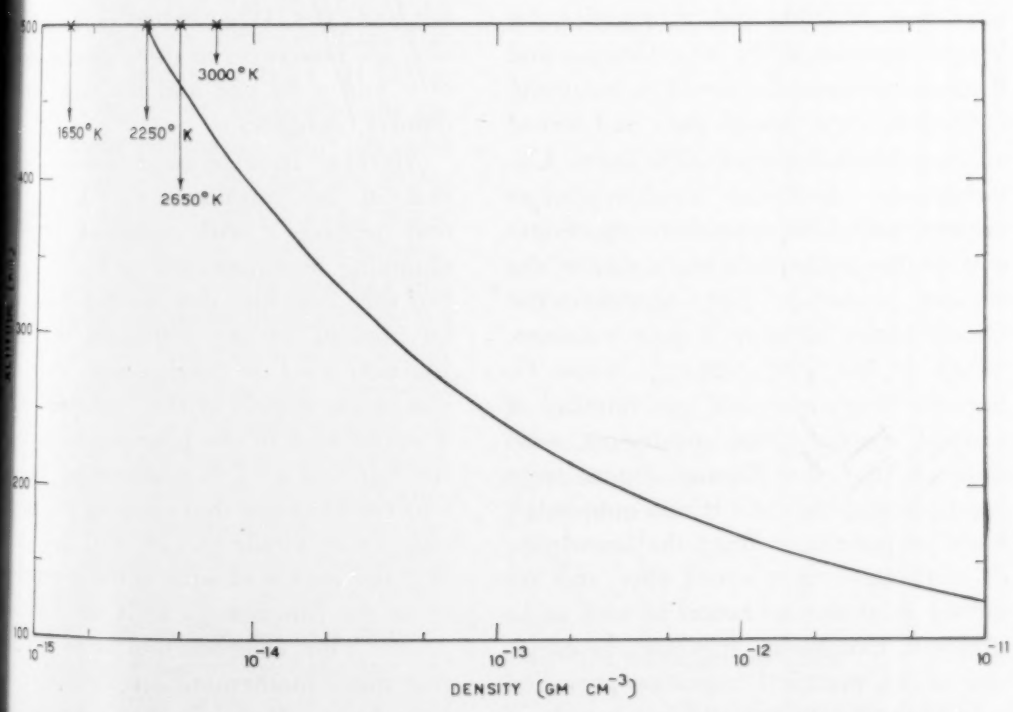

Fig. 5. The vertical distribution of the density can be determined when diffusion of constituents and conduction of heat are applied to the thermosphere. Density decreases relatively slowly at altitudes above 200 km. The crosses at 500 km show the effect of temperatures higher or lower than 2250°K on density values at 500 km.

Conclusions

The conclusions are these: (i) The atmospheric models of the thermosphere that neglect diffusion cannot explain ionospheric and satellite observations. (ii) The dissociation of molecular nitrogen cannot be introduced to decrease the mean molecular mass. (iii) The oxygen-

nitrogen ratio cannot be considered as constant. (iv) An atmospheric model must be founded on heat transport such as suggested by Chapman. (v) It is possible to estimate temperatures and temperature gradients in the neighborhood of 500 km and, consequently, conservative values of densities, if various values of the heat conduction are adopted.

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New Approach To Teaching Intermediate Mathematics

It is based on a resolution of the spectra of meanings of the letter *x* and the term *variable*.

Karl Menger

Nothing is more distasteful to an active mathematician or scientist than discussions of symbolism and notation, and that dislike is perfectly understandable. After having overcome in his youth whatever difficulties the formal expression of ideas presents, the mathematician finds that certain ways of writing have become his second nature and regards any suggestion of a change, even if he recognizes its merits, as nothing but a trivial nuisance.

There are, however, situations in which a thorough discussion of such matters on the highest level is inevitable. They occur when, at turning points in the history of culture, it becomes imperative to make certain techniques and ideas of mathematics available to wider strata of the population. In the large groups to be initiated, many persons lack the ability to overcome the difficulties that the specialist overcame in his youth. Moreover, an immense collective benefit results if even persons with that ability are spared unnecessary complications.

Dr. Menger is professor of mathematics at Illinois Institute of Technology, Chicago.

Such a turning point affected arithmetic when, during the Renaissance, mercantilism and experimental science were born. In banks and laboratories, the letters introduced by the Greeks and Romans as numerals proved to be utterly inefficient, even though they had served arithmeticians for over 2000 years. Unfortunately, medieval mathematicians misinterpreted the specialists' manipulative facility as intrinsic simplicity of the ancient numerals and regarded the Hindu-Arabic ideas as a pure nuisance. "Even in the 15th century," wrote G. Sarton, "there were still any number of learned doctors and professors who claimed that the Roman letters were much simpler than the Hindu numerals." Such prejudices confined the knowledge of arithmetic to a small elite and retarded its democratization as well as its progress. Eventually, however, as everyone knows, practical exigencies prevailed —incidentally, to the ultimate benefit of pure mathematics too.

The middle of the 20th century appears to be another such turning point. This is the time when scientific and technological progress has reached proportions necessitating the dissemination, on a large scale, of intermediate mathematics. A considerable part of the population should learn certain techniques of algebra, analytic geometry, and calculus as well as some basic ideas of those theories. The attempts toward this aim, which follow traditional lines, are generally regarded as not sufficiently successful. In my opinion, the principal stumbling block is the fact that most of those great mathematical ideas and techniques are being presented in their 17th century form.

Uses of x

A principal feature of those antiquated formulations is the indiscriminate use of the letter *x* (as well as of the letter *y*) in diverse meanings and according to discrepant rules. What enhances the confusion are references to those diverse types of *x* and *y* by one and the same term, namely, *variables*.

Algebra. In algebra, beginners learn that in the formula $x+1=1+x$ they may replace *x* with numerals, thereby obtaining formulas such as $4+1=1+4$. But they find that this practice must not be applied to the statement that the function $x+1$ is nonconstant, since replacement therein of the "variable" with 4 would lead to the false statement that the function $4+1$ is nonconstant. Beginners further learn that squaring the equation $y=x^4$ yields $y^2=x^8$. But they find that the square of what often is referred to as the function $y=x^4$ is the function $y=x^8$. This contradiction is so blatant that many mathematicians altogether refrain from referring to the said functions as $y=x^4$ and $y=x^8$, and rather call them briefly, the functions x^4 and x^8. As a result, however, *x* frequently has various

nings in one and the same statement. example is the following sentence:

(S_1) The function x assumes the value x for any number x.

is statement (S_1) is being inculcated beginners until many of them get to it. Getting used to it means realizing that the function x does not assume itself as a value (even though it assumes value x), that it is not any number (even though x stands for any number), and so on. In other words, accepting statements of the type of S_1 means realizing that they must not be taken literally. Therefore, there are also many beginners who give up. They have heard so much about the perfect precision of the language of mathematics that, after failing to understand some mathematical statements when taken literally, they regard themselves as lacking any mathematical ability. This group includes persons of great intelligence.

Analytic geometry. In analytic geometry, the beginner learns that a certain parabola is the locus of all points (x,y) such that $y=x^2$ or (which is the same) of all points (a,b) such that $b=a^2$. For instance, this parabola includes the point (3,9) but not the point (9,3). Indeed, $9=3^2$ but $3 \neq 9^2$; in other words, if $a=3$ and $b=9$ then $b=a^2$, whereas if $a=9$ and $b=3$ then $b \neq a^2$. Of course, that parabola may also be said to be the locus of all points (b,a) such that $a=b^2$. This locus, too, includes (3,9) but not (9,3) since $b=3$ and $a=9$ imply $a=b^2$ whereas $3 \neq 9$ and $a=3$ do not. All this is in no way surprising. But ask the following question: May this same parabola also be described as the locus of all points (y,x) such that $x=y^2$? Of course, the answer is again affirmative. For instance, this locus includes (3,9) but not (9,3), since $y=3$ and $x=9$ imply $x=y^2$ whereas $3 \neq 9$ and $x=3$ do not. Yet this affirmative answer would utterly bewilder the beginner. He would be unable to reconcile it with the fact (likewise taught in analytic geometry) that the parabolas $y=x^2$ and $x=y^2$ are altogether different. The teacher's only hope is that the question mentioned will not be raised and, therefore, that the apparent contradiction will remain unnoticed. For within the classical frame of concepts it is impossible to explain that paradox, the explanation being that mathematicians traditionally use the same pair of letters x, y in discrepant meanings when talking about the parabola $y=x^2$ and about the locus of all (x,y) such that $y=x^2$. That in the latter case one may interchange x and y while in the former one must not is a mere symptom of conceptual differences which traditionally remain inarticulate.

Calculus. In calculus, the reciprocity of differentiation and integration—the very core of the theory—traditionally is expressed as follows:

$$\frac{\mathrm{d}}{\mathrm{d}x}\int_a^x f(x)\,\mathrm{d}x = f(x) \qquad (1)$$

for any continuous function $f(x)$ and any number a. Serious shortcomings of formula 1 become apparent in manipulating symbols, even though it has been frequently claimed that the classical symbolism (while perhaps obscuring some of the contents) certainly facilitates manipulative use. In its five occurrences in formula 1, the letter x follows altogether discrepant rules. It may, without any change of the meaning, be replaced with any other letter in its last two occurrences on the left side of formula 1 or in its other three occurrences. For instance, the formulas

$$\frac{\mathrm{d}}{\mathrm{d}x}\int_a^x f(t)\,\mathrm{d}t = f(x)$$

and

$$\frac{\mathrm{d}}{\mathrm{d}u}\int_a^u f(x)\,\mathrm{d}x = f(u)$$

have exactly the same meaning as formula 1. In contrast, formulas resulting from other replacements, such as

$$\frac{\mathrm{d}}{\mathrm{d}u}\int_a^x f(x)\,\mathrm{d}u = f(x)$$

and

$$\frac{\mathrm{d}}{\mathrm{d}u}\int_a^u f(x)\,\mathrm{d}u = f(u)$$

are, in general, incorrect. The letter a in formula 1 may be replaced with a numeral as in

$$\frac{\mathrm{d}}{\mathrm{d}x}\int_1^x f(x)\,\mathrm{d}x = f(x). \qquad (2)$$

No such replacement of x in one or more of its occurrences yields a valid formula. For instance,

$$\frac{\mathrm{d}}{\mathrm{d}4}\int_1^4 f(4)\,\mathrm{d}4 = f(4)$$

is utterly nonsensical, while

$$\frac{\mathrm{d}}{\mathrm{d}x}\int_1^4 f(x)\,\mathrm{d}x = f(4)$$

is, in general, false. He who wishes to state that the two functions equated in formula 2 assume equal values for $x=4$ may replace x with 4 only in its last occurrence. What he must write is:

$$\left(\frac{\mathrm{d}}{\mathrm{d}x}\int_1^x f(x)\,\mathrm{d}x\right)_{x=4} = f(4). \qquad (3)$$

In view of such examples, one may well question the traditional claim that the classical presentation is conducive to a purely mechanical handling of symbols.

Needless to say, these complications do not present the slightest difficulties to anyone who has mastered a traditional course in calculus. The reason for mentioning them is the present vital interest in *increasing* the number of people who master the ideas and manipulative techniques of calculus (though not necessarily in their 17tth century form), reminiscent of the Renaissance interest in increasing the number of men able to perform multiplications and divisions (though not necessarily in Roman numerals). True, the time-honored formulation (formula 1) of the Reciprocity Law, which goes back to Leibniz, has been successful during the three centuries that witnessed the activities of Euler, Gauss, and Poincaré. But the Greek numerals were successful during the eight centuries from Pythagoras to Archimedes to Diophantos.

Questions

In view of the quoted (and countless other) examples from intermediate mathematics, what is remarkable is how well teachers succeed in transmitting to many students a feeling for what is right in manipulating x and y and an instinct for which type of variable is present where. (Since the underlying distinctions do not attain the level of the articulate, feelings and instincts are all that can be transmitted.) That, notwithstanding all the teachers' efforts, many beginners, including talented students, give up, is not surprising. Clearly, various circumstances contribute to the unfortunate and dangerous situation in our current mathematical education. But the antiquated symbolic and conceptual frame in which mathematics is being presented certainly is in itself a sufficient reason.

The question naturally arises why these difficulties should come to a head just in this country and at this time. One obvious reason is our attempt to initiate a much higher percentage of the population into intermediate mathematics

than do the countries in Central and Western Europe. But there are psychological, in addition to sociological, factors which I carefully studied when, after teaching in various parts of the European continent west of Russia, I initiated thousands of American undergraduates into intermediate mathematics during the war and the G.I. period, and taught hundreds of adults in a metropolitan night school. From these vantage points, for the past 15 years, I have collected the questions that beginners actually ask and have noticed that the same questions are asked again by mature men who did not receive satisfactory answers in their youth—a fact on which they blame the superficiality of their mathematical knowledge. Almost all of those questions concern the symbolic and conceptual frame of intermediate mathematics. They are raised more frequently on this side of the Atlantic because the American youngster approaches the subject with pure common sense and utterly rejects dictatorial solutions. On the other hand, he less enjoys intellectual acrobatics and easily gives up when dissatisfied. Within the traditional frame, unfortunately, most of those questions are simply unanswerable.

New Presentation

It has been in response to these questions that, for the past 15 years, I have developed a new presentation of intermediate mathematics, outlined in numerous papers (*1*) and elaborated in two textbooks (*2*), which I have tried out in teaching hundreds of students. The essence of this presentation is an approach to mathematics that is based on common sense, which thereby furthers the understanding of the material, and which, moreover, results in truly mechanical manipulations.

1) Emphasis is laid on the possibility of talking about mathematical objects and their interrelations somewhat as one talks about people and their family relations, as in the sentence: The father of the paternal grandfather of a person is the paternal grandfather of the father of that person. The words "a person" delimit the scope of the assertion and make clear that it is being proposed about all persons and not only, say, about all living men, or, on the other hand, about all mammals. If f, g, and = were generally accepted symbols for the words "the father of," "the paternal grandfather of," and "is the same as," respectively, then one might write:

$$fgX = gfX \text{ for any person X.}$$

But the letter X in the formula is not self-explanatory and does not delimit its scope. This is why the formula, in order to render the sentence, must be amplified by the explanatory legend "for any person X." Similarly, the square of the fourth power of a real number equals the fourth power of the square of that number. The words "a real number" delimit the scope of the assertion and make it clear that it is being claimed for all real numbers and not only, say, for all integers, or, on the other hand, for all in some way generalized numbers. Using universally accepted symbols and a letter in lieu of the words "a real number," one may write

$$(x^4)^2 = (x^2)^4 \text{ for any real number x.}$$

Since the letter x is not self-explanatory, the mere formula would again leave uncertainty about the scope of the assertion, wherefore it must be amplified by a legend. Even the very intent of a mere formula including a letter is in need of clarification. One and the same formula occurs, for instance, in the *assertion* that $x^2 - 9 = 0$ for x being 3 or -3, and in the problem: find x such that $x^2 - 9 = 0$. In case of an imperative legend, no assertion is intended, and the letter is referred to as the *unknown* of the problem, whereas the letter in a formula accompanied by a description of its scope is called a *variable,* more specifically, a *numerical variable.* This is the only sense in which the latter term is used in the new presentation. If the writer of a formula that includes letters not designating specific mathematical objects fails to append a legend explaining the intent and the scope of the formula, then he forces his reader to do mere guessing—a procedure strictly shunned in the new approach.

2) The area (say, in square feet) of a square is the second power of the length (in feet) of the side of that square or, in a formula following these words:

$$a(Q) = s^2(Q) \text{ for any square Q.} \quad (4)$$

Here, Q serves as what might be called a square variable. In contrast, a and s designate definite mathematical objects of the type that Newton called *fluents,* namely, area and length in the realm of squares, each fluent resulting from the association of a number with an object of a certain kind. Traditionally, formula 4 is abbreviated to the formula $a = s^2$ connecting the two fluents themselves rather than their values for any square—a situation unfortunately obscured by referring to the fluents a and s as *variables* and thereby adding another meaning to that highly equivocal term. Naturally, a and s must not, in formula 4, be replaced with letters designating any two other fluents (say, perimeter and diagonal), nor should they be interchanged. While $a = s^2$ is true, $s = a^2$ is false, just as, in the realm of numbers, $e < \pi$ is true and $\pi < e$ is false. Contrast $a = s^2$ with a statement about many numbers; for example, for any two positive numbers, a and s,

$$\text{if } \sqrt{a} = s, \text{ then } a = s^2.$$

Here, a and s do not designate fluents. Here, they serve as numerical variables and may, without any change of the meaning, be replaced (for example, with x and y) or even interchanged: for any two positive numbers a and s,

$$\text{if } \sqrt{s} = a, \text{ then } s = a^2.$$

In the clarified presentation, the conceptual difference between numerical variables and fluents is visibly reflected in a typographical distinction that the reader will note in paragraphs 1 to 5 of this section. Letters in roman type serve as numerical variables, while fluents and functions are designated by *italic* type. This device not only greatly facilitates intelligent reading of mathematics but forestalls a great deal of otherwise almost inevitable confusion. The class of all points (that is, pairs of numbers) (x,y) such that $y = x^2$ is the same as the class of the pairs (a,b) such that $b = a^2$ or of the pairs (y,x) such that $x = y^2$. On the other hand, the parabola $y = x^2$, that is, the class of all points P such that $y(P) = x^2(P)$, is of course different from the parabola $x = y^2$. Here, x and y are fluents, the abscissa and the ordinate whose values for the point P are $x(P)$ and $y(P)$. None of the paradoxes of the 17th century notation has to be explained, because in the new presentation none of them ever arises.

3) The (traditionally symbol-less) identity function, which for any number x assumes the value x, is a mathematical object of paramount importance, and it clearly deserves a permanent symbol. If j is used as its designation, then the obscure statement S_1 is replaced by:

j is the function such that, for any number x, the value $j(x)$ equals x.

Every statement made in the course of the new presentation may be—in fact, must be—taken literally. The function $j \neq 1$ is nonconstant, and $(j^4)^2 = j^8$. Here again it might be argued that since the lack of a symbol for the identity function has not impaired the success of analysis for the past 300 years, such a symbol must be superfluous. But, at about A.D. 300, Greek mathematicians could say that the lack of a symbol for zero had not impaired the success of their arithmetic for over 800 years. Yet the introduction of the Hindu cipher 0 made arithmetic even more successful and greatly furthered the development of algebra.

4) The distinction between numerical variables, fluents, and functions entails the distinction in calculus between the derivative of a function and the rate of change of one fluent with respect to another fluent—two terms traditionally considered as synonymous even though the derivative associates a function with a function, and the rate of change associates a fluent with two fluents. For instance, the derivative of the sine function is the cosine function (the symbols for the sine and cosine functions are italicized, whereas x serves as a numerical variable):

$$\mathbf{D}\ sin = cos,$$
$$\text{or } \mathbf{D}\ sin\ \mathrm{x} = cos\ \mathrm{x} \qquad (5)$$

for any x. The rate of change of the distance traveled with respect to the time elapsed is velocity:

$$\mathbf{d}s/\mathbf{d}t = v.$$

For a harmonic oscillator,

$$\text{if } s = sin\ t, \text{ then } \mathbf{d}s/\mathbf{d}t = cos\ t. \qquad (6)$$

Here s and t are specific fluents in contrast to the numerical variable x in formula 5. If s and t in formula 6 are misused as numerical variables, say, by replacing t with π, and s with 0, the result is an implication whose antecedent ($0 = sin\ \pi$) is valid, while its consequent, $\mathbf{d}0/\mathbf{d}\pi = cos\ \pi$, is utter nonsense. The derivative of a function is its rate of change with respect to the identity function: $\mathbf{D}f = \mathbf{d}f/\mathbf{d}j$, for any differentiable function f. The situation in integral calculus is analogous.

5) In the new presentation, symbols for operations and operators are introduced with great care, avoiding synonyms and equivocations, and in a way that is free of confusing ballast. For instance, the integral beginning at 1 of the function f might be denoted by $\mathbf{S}_1 f$, following the verbal pattern. This symbol bears a relation to the synonymous traditional symbols

$$\int_1^x f(x)\,\mathrm{d}x \text{ and } \int_1^x f(t)\,\mathrm{d}t$$

somewhat like that of "1984" to "MCMLXXXIV" and "MDCCCCLXXXIV." The introduction of any symbol is accompanied by articulate rules concerning its use. In particular, clear stipulations are made as to which part of a formula where that symbol appears is within its reach. One of them is the stipulation that within the reach of an operator symbol (all of which are printed in bold face, as **D** and $\mathbf{S}_1$) is only the immediately following function. On this basis it is clear that **D** *sin* π is the value that the function **D** *sin* assumes for π, and not the derivative of the (constant) function *sin* π, which would be denoted by **D** (*sin* π). Such rules, in conjunction with the use of a symbol for the identity function, make it possible actually to achieve what the classical treatment claims to achieve: to *manipulate formulas in a purely mechanical way*. In the traditional transition from formula 2 to formula 3, like letters in various occurrences are treated in altogether unlike ways. In contrast, the streamlined version of formula 2—that is,

$$\mathbf{D}\,\mathbf{S}_1 f = f \text{ for any continuous } f \qquad (2')$$

—implies $\mathbf{D}\,\mathbf{S}_1 f\mathrm{x} = f\mathrm{x}$ for any x; in particular,

$$\mathbf{D}\,\mathbf{S}_1 f4 = f4. \qquad (3')$$

The transition from general statements to specific formulas proceeds by systematic substitutions and by replacements of variables with designations of specific objects. This technique results not only in a simplification of pure as well as applied analysis but in their complete standardization and automatization.

Conclusion

It goes without saying that, when initiating students into the great ideas of 17th century mathematics in any reformed presentation, one must not neglect to teach them to read the various classical notations, especially those going back to Leibniz, to Lagrange, and to Cauchy. (Our Renaissance ancestors, when disseminating the ideas of arithmetic in the reformed symbolism, taught their students also to read Roman numerals, which in fact are still being taught.) It has been my experience that this aim can be achieved without difficulty. Larger scale experiments would undoubtedly result in further improvements.

The main problem clearly lies in the instruction of *teachers* who have been brought up to regard as nonexistent just those points that cause the beginners' crucial difficulties, and who themselves have never been provided with answers to their students' basic questions—the questions concerning the antiquated frame of intermediate mathematics—even though many teachers feel that those questions are justified. In other words, the problem is *to instruct those teachers to use symbols and basic concepts consistently and to transmit to their students the clarified techniques according to articulate rules.* Considering the remarkable, if partial, success of teachers along traditional lines, one may be confident that, equipped with adequate conceptual and symbolic tools, they will make intermediate mathematics available to such wider strata of the population as the present age demands (*3*).

References and Notes

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2. K. Menger, *Calculus. A Modern Approach.* (Ginn, Boston, 1955) and *Basic Concepts of Mathematics. A Companion to Current Textbooks in Algebra and Analytic Geometry.* (Illinois Tech Bookstore, Chicago, 1957.)
3. The writing of this paper is part of the work made possible by a grant from the Carnegie Corporation of New York for the development of a new approach to the teaching of mathematics.

A Human Enterprise

Science as lived by its practitioners bears but little resemblance to science as described in print.

Harold K. Schilling

In one of his essays, Herbert Dingle (*1*), historian and philosopher of science, makes the following remark: "When we contemplate the ideas of the essential nature of science which are most prevalent and operative today we find a situation fit to make the angels weep." Without doubt this is true. "Science" as it is thought of popularly is a stereotype that bears but little resemblance to science as it is known intimately by those who live it from day to day.

For instance, it is commonly believed that science is a sort of intellectual machine, which, when one turns a crank called "the scientific method," inevitably grinds out ultimate truth in a series of predictably sequential "steps," with complete accuracy and certainty. Its thinking is thought of as exclusively and inerrantly logical in the most formidable sense, and its language, as utterly precise and unambiguous. In view of such magic omnipotencies that presumably make all wrong answers impossible, science is regarded as a monolithic structure of unassailable truth and method on which all scientists must necessarily be agreed.

There are at least two reasons why such unfortunate utopian notions prevail. First, the findings of science are usually presented to students and the public as straightforward, logical developments, rather than in such a way as to reveal how they actually evolved—haltingly, circuitously, with many false starts, and often even illogically. While there are, of course, very good reasons for this, the fact is that, in the absence of further explanation, it leaves the uninitiated with a thoroughly misleading idea of the processes of science. Moreover, most conventional portrayals of science are, strictly speaking, not descriptions at all, but schematic interpretations. They are the product of a process of selective abstraction, by which the interpreter extracts from the complexities of actual science those elements that seem to him most typical and most capable of simple, systematic description or analysis. In this way he creates a simplified, idealized image or model, which the reader innocently accepts as an accurate portrait of science. For both these reasons, then, science as the scientist himself knows it remains essentially unknown.

The purpose of this article is to call attention to a few aspects of the real science, about which little seems to have been said in print.

Science, a Realm of Human Contrasts

A typical aspect of most conventional interpretations is their almost complete silence about the fact that science is a typically human enterprise with the limitations and potentialities, weaknesses and strengths these usually possess. One indication of this is that it is a realm of great contrasts and nonuniformities, a structure that is anything but monolithic.

Consider, for instance, the great difference between the science of the intellectual frontier and that of the interior. These are as different as the laws, politics, or social and economic structures on a national frontier are typically different from those behind the frontier. Frontier science is exploratory and adventurous. Here ideas are tentative and impermanent, coming and going rapidly. More often than not they are audacious guesses or vague hunches that rarely conform to established patterns of thought. Often they are thoroughly unorthodox and what many people would even regard as "unscientific." This is science in the raw —controversial, competitive, inefficient, governed to a considerable extent by the demands and urgencies of the moment, and employing predominantly *ad hoc* methods. It is the science of the restless explorer, always on the trek, never stopping anywhere very long—always looking for new horizons and taking the frontier with him.

The science of the interior, by contrast, is that of the intellectual colonizers who follow the pioneers, consolidate gains, and establish order and respectability. It is characterized by much less fluctuation and change, and therefore by relative permanence; by system, precedence, and closer adherence to established canons of methodology and thought; by logic more than by hunch. Here the crooked pathways are made straight. Here is where the straightforward "proofs" or "logical developments" put in their appearance, and where everything seems to be orderly and logically interdependent.

Now it is the teacher's and interpreter's preoccupation with this systematic, rational science of the hinterland that is largely responsible for the overabstracted, formalized stereotype to which I have been referring. Certainly, such an oversimplication as that of "the scientific method" could not possibly have arisen out of careful contemplation of frontier physics.

Another inhomogeneity of science appears in the tremendous contrast between the science of the great masters and that of the ordinary, common man of science. The conventional images of historical, as well as contemporary, science are the result of using glasses that bring into focus mostly the great towering figures of our science, leaving much of the picture unseen. To know America only in terms of its great heroes—George Washington and Abraham Lincoln, Ralph Waldo Emerson and William James—is to know it only partially. True understanding requires that it be known also in terms of the common man and his way of life. The same is true of science. To see it only as the creation of its towering geniuses—Galileo, Newton, Harvey, Pasteur, Einstein, Bohr, and Heisenberg—is to have a foreshortened view of it. There is also the science of the ordinary, garden variety of scientists. To see it in true perspective means to be aware of and understand science as the work and way of life of these, its lesser devotees —who are almost unknown except in their own localities or to fellow workers in their own particular narrow subfields

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cience, who are not at the very fore- of modern research, and who in a le lifetime publish only a few papers stricted significance, but who never- ess are real scientists.

Now to depict the common-man as- t of science clearly, and to demon- te what many of us feel—namely, t it is significantly different from the ter-known science of the masters— uld require much more factual knowl- e than is now available. I am rather e, however, that if this aspect of sci- e were studied carefully, we would be ed to revise rather extensively many the conventional descriptions and odels" of science—for there can be doubt that most current conceptions the operations and modes of thought science and scientists have resulted m a disproportionate preoccupation th, and abstraction from, the science the great masters. I suspect that such dies would make it abundantly clear w completely unsatisfactory any state- ent must be which began, say, with e scientist does it this way, but not t way," or "the scientist believes this, e that." It would be evident that there no such thing as "the scientist," that s term must necessarily stand for many nds of scientists, whose ways of think- g and habitual modes of experimenta- n and research differ widely, and mong whom there are many degrees of phistication with regard to the pur- ses, goals, and methodology of science d many fundamental disagreements out both the content and meaning of principles, concepts, and generaliza- ons. And especially, I suspect, such dy would reveal pronounced dissimi- rity between the patterns of intellec- al strategy and tactics prevailing in the mmon-man science and those of the eat-man science.

Historical analysis would probably re- al that much of the growth of physics the aggregate effect of the interests, titudes, professional habits, and contri- utions of the lesser men of physics. In all obability the meandering onward flow science is determined helpfully and sitively—and to a large extent—by the nk and file, who, because of their per- stent interests and preoccupations, carry xploration and exploitation in particu- r fields to their logical conclusions long fter the geniuses have lost interest and rned to other more enticing problems. t may show also that progress is aided eatly by the damping and filtering ef- ects of the intellectual inertia and skep- icism of the ordinary man of physics upon many of the exuberant, freewheeling, and less useful ideas of the great or near great. Finally, the sum total of the relatively less important research endeavors of the very large number of individual mediocre scientists is tremendous and probably accounts for most of science as measured by both input of energy and output of results. It is amazing how little of this is realized by most people outside of the sciences.

Science as a Social Enterprise

Science is not only a human, but more particularly a social, enterprise—that is, one of sharing, cooperation, and interaction of people. Among my students the following question has always generated considerable interest and discussion: Is a one-man physics possible? Could a man completely isolated from other people, but possessing great intelligence and ingenuity and unlimited material resources and time, eventually develop a physics like the one we know, which has developed by social action? Almost invariably the student replies that it would certainly be possible. The lone scientist could, it is argued, make observations and generalize from these. Then he would discover the need for experiment and measurement, and for instruments. Learning that the use of these enabled him both to reduce personal errors and to increase his output, he would then use microphones as much as possible instead of his ears, photoelectric cells instead of eyes, analyzers and computers to augment his brain. Finally, he would build laboratories with complete automation and, in time, obtain all the data and curves required to establish all the functional relationships and generalizations of physics.

Now this kind of answer comes out of the prevalent notion that physics is a dehumanized science that owes its success almost exclusively to instrumentation and measurement. It is the kind of answer one gets from people who have had little direct contact with physics. Experienced physicists, however, give a different answer. They point out that unless our lone physicist were a very different kind of human being, he would be in need, whether he realized it or not, of checking and confirming his data by those of other physicists; otherwise he might go on blissfully unaware of serious shortcomings and systematic errors affecting his instrumental readings and settings and therefore his results—even if there were automic controls. In other words, physics as we know it requires checks and balances, and mutual validation and verification.

Consideration of such hypothetical, artificial, dehumanized situations brings into the open many facts about the real, existential science that rarely are noticed or mentioned. Thus there are, of course, no supermen who can go it all alone, and probably there are none who would want to go it alone if they could. Not only does the scientist always have his limitations, necessitating mutual aid, but he knows it. Not only does he need help in making experiments and interpreting their results, but he is conscious of that need. The recognition of it constitutes part of his professional equipment. Moreover, sooner or later he wants to exchange and share new ideas and findings with fellow scientists. He knows that if there is to be progress he must build on the results of others and must, in turn, make contributions upon which others can build. He realizes that he needs the criticism of his fellows, but also he craves their approval. Few scientists would do research, I believe, if they could not publish their results and get due credit for them, or could not see socially beneficial consequences flowing from their research, or were not motivated socially in other ways that certainly are operative in our present science enterprise. Science is undeniably social.

It is in this social mutuality and interdependence that the so-called "objectivity" of science has its roots. As is well known, the potency of the sciences in their search for knowledge and truth lies to a large extent in their insistence upon empirical evidence and confirmation. It is much less well known that in this connection the word *empirical* refers primarily to social rather than solitary individual experience, observation, and experimentation. A term that is highly suggestive of basic meanings here, and which may be regarded as a synonym of *objectivity,* is *intersubjective testability* (*2*). It calls attention to the fundamental role played by interpersonal exchanges and checks in the testing and confirmation processes of science.

Science Is Communal

Not only is science human and social, as I have just suggested, but it is also definitely communal. Without doubt the term *science community,* heard with increasing frequency, is extremely useful

in describing science as it actually is. Certainly it does exist—and it is a community with the usual attributes of human communities. It has it own ideals and characteristic way of life; its own standards, mores, conventions, signs and symbols, language and jargon, professional ethics, sanctions and controls, authority, institutions and organizations, publications; its own creeds and beliefs, orthodoxies and heresies—and effective ways of dealing with the latter.

This community is affected, as are other communities, by the usual vagaries, adequacies, and shortcomings of human beings. It has its politics, its pulling and hauling, its pressure groups; its differing schools of thought, its divisions and schisms; its personal loyalties and animosities, jealousies, hatreds, and rallying cries; its fads and fashions.

The life and operation of this community require an amazing number of different kinds of people and talents. These include at least the following: the experimentalist and theorist, the lone researcher and team researcher, the critic and referee, the philosopher and historian of science, the teaching scientist, the research director, the research manager and business officer, the personnel officer, the report writer, the editor, the translator, the liaison officer, the instrument designer and maker, and—last, but not least—the forsaken "science widow" who keeps the soup warm when her husband is late, or who spends innumerable, endless nights alone while he is pursuing a hot lead in the laboratory. Similarly, the community is supported by an array of personal motivations as varied and extensive as the gamut of talents just referred to. Here, too, one encounters a thoroughly human situation. The underlying drives that lead men to become scientists, or that determine their decisions thereafter, are by no means confined to those commonly associated with ivory-tower conceptions of science or with pure unselfish love of the truth. It is doubtful if science could survive if this were not so.

As I have already said, this community has its own unique way of life and dominating interests that offer their own satisfactions. All this is very hard to describe, and nearly ineffable—though nonetheless real. There is something intimate about it, something shared and deeply felt, though unspoken. It can be understood truly only from within the community. An interesting question arises here: How do physicists, say, recognize each other as belonging to the physics community? I suggest that a physicist identifies a fellow physicist by rather subtle clues. Although he has certain definitions of physics, and therefore of the term *physicist*, none of these is really wholly satisfactory for making decisions in many actual situations. I would assert that basically, in the last analysis, he does not make the judgment that a man is or is not a fellow physicist by Aristotelian logic or careful definition and analysis, but rather on the basis of feelings, and the vague, unspoken, intimate, indescribable, but deeply felt intangibles of unformulated common interest, purpose, and attitude that become compellingly real to those who participate in the shared experience and life of the physics community.

This is no mere empty, useless sentimentality. It is utterly realistic and intensely practical. How, by way of illustration, does an editorial board of a physics journal proceed when a paper of high quality is submitted for publication, which they think deals with chemistry rather than physics and should appear in a chemistry journal? Does their judgment result from the conscious, logical application of formal definitions or criteria? By no means. While, to be sure, they may give some thought to more formal considerations, I believe that their decision rests basically on translogical and undefined feelings, on insights, intuitions, and a sense of values they have developed for the most part unconsciously as they have lived and gone about the business of physics as members of the physics community.

It is to this kind of intuitive thinking that many physicists finally resort, after all other reasoning fails, when they are called upon to say what is physics, or what is the difference between physics and chemistry. Many a long, animated, informal discussion—even formal committee deliberation—has yielded the profound conclusion that physics is what physicists do professionally, and that physics is what goes on in the physics building and chemistry, what goes on in the chemistry building of a university. And by means of this truly profound conclusion, many other practical professional questions are answered.

The science community is deeply embedded in the world of affairs and has always been influenced profoundly by other components of society and by human needs and demands (*3*). Contrary to prevalent opinion, science has not risen above its environment by becoming independent of or immune to external influences or pressures. It has interacted with its environment with profound effects, and often its own reactions have been conscious and deliberate.

One of the most profound effects of cultural influences upon science has been, surprisingly, in the area of decision making—with regard to strictly scientific questions. Much historical research has shown that choices within science itself have often been based on considerations that, from present-day points of view, seem much more appropriate to other disciplines and areas of life (*4*). There can be no doubt that the reasons for accepting hypotheses, theories, concepts, modes of thought within science have often been political, social, economic, philosophical, or theological in nature and origin.

Science is increasingly being regarded as a quest for the rationalization or understanding of certain aspects of human experience—rather than only as an exploration of an external world. From this point of view science is human, not only in its characteristics but also in its most basic concern—namely, the object of its inquiry. We have come to realize, as perhaps no scientists before us ever have, that the human observer or explorer and his experience are integral and determinative parts of whatever world he is studying.

Need for More Adequate "Models"

In conclusion, I should recognize the point of view of many persons who would say that when scientists follow their feelings and hunches rather than formal logic, or accept hypotheses for political rather than strictly "scientific" reasons, they are merely mistakenly letting their humanity intrude into their science, and that "science itself" is something apart from any such intrusions. While this may be regarded as only a question of definition, I submit that it is much more than that—namely, a matter of understanding science adequately in its actualities. A "science-as-such" is, after all, only a mental construct. As ordinarily conceived, it does not correspond sufficiently to the realities of science in the concrete to be adequately descriptive. I would plead that it is much more useful, fruitful, and enlightening from many points of view to conceive of science as I—(and many others) have—namely, as being broadly pervasive and widely inclusive, comprehending within its purview much of life

at by other definitions would be re-
rded as unscientific. I would plead
at philosophers, teachers, and other in-
preters who construct and employ
odels of the scientific enterprise so con-
ruct them as to represent more ade-
ately and explicitly the great diversi-
s and nonuniformities of science, and
ny more of its actualities, than most
the conventional current ones do. If
e public is to understand and appre-
iate—as well as intelligently support—
ience, it must have a more inclusively
truthful picture of it than it now possesses. If, in planning for the future, we are to project for science a truly significant function in public affairs, we must base our thinking about how it should operate in the future upon a model that depicts as accurately and inclusively as possible how it does in fact operate now. So far as I am aware, such a model, or image, does not now exist. Our thinking has been dominated altogether too much by a stereotype that is thoroughly inadequate and misleading.

References and Notes

1. H. Dingle, *The Scientific Adventure* (Pitman, London, 1952), p. 4.
2. It is my impression that this term was first proposed by Herbert Feigl. At any rate it appears in his essay "The scientific outlook: naturalism and humanism," *Am. Quart.* 1 (1949). See also Feigl and Brodbeck, *Readings in the Philosophy of Science* (Appleton-Century-Crofts, New York, 1953), p. 11.
3. B. Barber, *Science and the Social Order* (Free Press, Glencoe, Ill., 1952); H. Butterfield, *The Origins of Modern Science* (Anderson, London, 1951).
4. See the following papers and their references: P. G. Frank, *Sci. Monthly* 79, 139 (1954); B. Moore, Jr., *ibid.* 79, 146 (1954); A. A. Koyré, *ibid.* 80, 107 (1955); R. S. Cohen, *ibid.* 80, 111 (1955).

Tatuo Aida, Geneticist

Tatuo Aida, Japanese geneticist well known for his studies on the fresh-water fish *Oryzias* (*Aplocheilus*) *latipes,* died on 6 December 1957 at the age of 86. He was born in Kyoto on 21 November 1871, the only son of Masatoyo and Moto Aida, and was educated in the Third State Junior College in Kyoto and later in the Tokyo Imperial University, where he majored in zoology and graduated in 1896. His main interest at that time was in the pelagic invertebrates of the groups Chaetognatha and Appendicularidae. His Japanese and English papers on the former group, published in 1897, dealt with 12 species, of which four were new, and his English paper on the latter group, published in 1907, included 12 species of which four were reported as new. These papers were the first reports of these groups from the Pacific waters.

After his two postgraduate years in the university, he was appointed professor of biology in the Fifth State Junior College in Kumamoto. In 1904 he was called back to Kyoto by the death of his father and remained there until the end of his life. He taught biology in the Kyoto Higher Technical School as well as in a Buddhist school in the same city.

About 1913 he became interested in the genetic studies of *Oryzias,* varieties of which are commonly kept in Japanese homes, and he kept on breeding this fish experimentally in his home in the city of Kyoto. His garden was traversed by small meandering canals which provided clean water for his nursery. He used concrete tanks and earthenware basins for the pedigree cultures. His time, during the breeding season of the fish, was devoted almost entirely to the experiments.

The results of these seven years of painstaking work were embodied in his first paper, published in 1921 in *Genetics.* The most important finding described in that paper was the presence of a gene for red color, carried in the Y-chromosome, and its occasional transfer into the X-chromosome by crossing-over. This discovery was antagonistic to the then-accepted knowledge of the structure of the Y-chromosome, especially with respect to *Drosophila,* and Aida hesitated considerably to publish it. The discovery was sustained by the result of Schmidt's work on another variable freshwater fish, *Lebistes,* conducted in Denmark and published almost coincidentally with Aida's paper. Aida's finding, as was rightly pointed out by the editor of *Genetics,* E. G. Conklin, went beyond Schmidt's in having demonstrated crossing-over between the Y- and the X-chromosomes.

Aida kept on with experimental breeding of the same fish after the appearance of this classic paper and published two more papers in the same field. The second paper, in 1930, dealt with the findings on the frequency of crossing-over between X and Y and the apparent nondisjunction of the X-chromosome. In 1932 he was awarded the Japan Academy Prize for the excellence of his genetic studies on this fish.

The third paper, published in 1936, was on sex-reversal, which is relatively common in this fish. These two papers, as well as the first one, were the outcome of his laborious, long-continued experiments. His interest in the experiments never waned, even on his deathbed, and whenever he felt better, he got up to perform some experiments. Thus, he left rather extensive breeding results unpublished, and we are hoping that someone will examine his notebooks and publish his further discoveries in an appropriate form.

Aida had a robust physique and enjoyed good health until he contracted, in his 80th year, a fatal asthma. He had the well-controlled temperament of a samurai, and, in spite of his apparent shyness, he was a man of great versatility. For many years, as a consultant to the Shimazu Factory in Kyoto, he practically directed extensive business works in its department of natural history, manufacturing and selling specimens, models, and instruments to schools all over Japan, as well as in China, Korea, and elsewhere. He was interested, as much as in the breeding experiments with fish, in old Japanese swords; he had a great deal of experience in judging the quality, and determining the maker, of such swords and became an authority in this line. He was also a good archer and was ranked among the few champions who were able to shoot a target through the Thirty-three-ken (Sixty-yard) Corridor, in the traditional tournament among the best archers in Japan.

Tatuo Aida disliked publicity, so much so that he never took any doctoral degree, and his death, announced to his friends only some days after the private funeral, was not reported even in the local papers. We have lost in him a geneticist of outstanding ability and originality.

TAKU KOMAI

Kyoto, Japan

News of Science

Sputnik III

A third artificial earth satellite was launched in the Soviet Union on 15 May. Sputnik III, which weighs 2925 pounds, is a cone-shaped object that is 5 feet, 8 inches in height. The sputnik's orbit is inclined to the plane of the equator at an angle of 65 degrees. Initial reports placed the vehicle's apogee at 1167.4 miles. It takes approximately 106 minutes to circle the earth, and carries radio equipment that transmits signals on a frequency of 20.005 megacycles. The power supply includes both electrochemical and solar batteries.

At a press conference in Moscow, Yevgeni Federev, a member of the Soviet Union's International Geophysical Year Committee, reported that the variety and complexity of the instruments aboard the new satellite were its outstanding feature. Chemical fuels were used in the launching, and the launching technique was in general the same as that used for the first two Soviet satellites.

There are three major groups of instruments aboard sputnik III. The first includes apparatus designed to study cosmic phenomena such as solar radiation, cosmic rays, and the number and intensity of micrometeorites flying through space. Federev said the Soviet instruments in this category were "considerably improved" over earlier models.

The second group of instruments includes those for studying conditions in the earth's atmosphere—composition, pressure, ionization, electrical phenomena, and the strength and variations of the earth's magnetic field. Federev observed that this is "the first time in history" that it has been possible to have instruments recording all aspects of the atmosphere at all levels in the same "laboratory."

The final group of instruments aboard sputnik III are used for regulating the others—the conditions in which they operate and their sending of information to earth. In addition to the radio transmitter, these include temperature regulators and a program-control system that turns the various instruments on and off at the right times.

Federev also disclosed that the machinery for studying the satellite, its trajectory, and conditions of flight, as well as the scientific data it is collecting, has also been expanded. The number of Soviet observation stations has been increased by one-third.

Federev would not predict how long sputnik III would revolve around the earth. However, he commented that it was his "personal opinion" that sputnik III would stay up several months "and longer than the second sputnik." He also said its instruments would have a longer life than those on the second vehicle.

Chick Head Transplants

Mira Pavlovic, research associate in zoology at Yale University, has successfully transplanted the heads of chick embryos. The longest period of survival for a chick with a transplanted head was 70 days. Another lived 55 days. Other chicks stayed alive 9 days, 4 days, and 2 days, and one died right after hatching.

Miss Pavlovic, a Yugoslav scientist who has been at Yale since March 1957, has been working under John S. Nicholas, Sterling professor of biology. She has been using a transplant technique developed by Petar N. Martinovitch, another Yugoslav who held a Sterling research fellowship at Yale last year. Although Martinovitch developed the transplant technique, none of the chicks on which he performed the operation while in this country hatched. Since his return to Yugoslavia, however, he has succeeded in hatching a chick that lived about 2 days.

Miss Pavlovic has performed the operation on 100 embryos. Six of these embryos developed into hatched chicks. About 30 percent died on their last day of incubation.

The transplant is made within 33 to 40 hours after incubation, before the chick embryo's circulation system is established. The operation consists of opening a square in the shell of the egg. A pair of specially adapted watchmaker's forceps is used to cut through the mid-level of the embryonic mesencephalon. The detached tissue is then removed with a suction pipette and is placed directly on the previously prepared graft bed in the host, another chick embryo which has been decapitated. The window in the shell of the egg is then covered with transparent material, and the egg is placed in an incubator.

The experiments were performed on Rhode Island Reds, to which the heads of a black breed were transplanted. The bodies of the mature chicks were the color of the Rhode Island Red, while the tops of their heads were black. The region of the head that is transplanted starts roughly between the upper and lower beak of the chicken, and involves the eyes and ears, the forebrain, and half of the mid-brain. Examination of the chicks that matured showed that in some cases the upper and lower levels of the beak did not match.

The two older chicks, ones that lived 70 and 55 days, both were smaller than other normal chicks that were hatched at the same time and used as controls. There has been no detectable difference between the behavior of the chicks that underwent the transplant operation and the controls.

The study was made to see if the transplant caused any discernible difference in the nervous system of the chicken. The work seems to show that early in embryonic development the tissues of differing hosts are compatible. Apparently differentiation of tissues does not take place until after the development of the circulatory system.

Churchill College at Cambridge

Sir Winston Churchill has urged the establishment of a men's college at Cambridge University to train an elite corps of scientists and technologists. On 14 May Churchill issued an appeal to his countrymen to contribute to the founding of the college, which would bear his name. He is already chairman of its board of trustees.

The new institution, according to a letter from the other trustees, is intended to produce "leaders trained at the highest level in science and technology." Seventy percent of its members would study scientific and technological subjects. There would be a higher proportion of postgraduate students than in existing colleges: one to every two undergraduates. A number of visiting fellowships would be created for specialists from abroad, and the college would try to attract postgraduate students from foreign countries and the Commonwealth.

Sir Winston's appeal emphasized the "vast significance" of technological progress to the United States and the British Commonwealth and commented: "It is a theme on which the English-speaking peoples can and must work in concert disregarding national boundaries and

king unity in the benefits their joint forts can offer to all men."

A minimum initial enrollment of 200 250 students and 30 to 40 fellows is anned, and it is expected that there uld be an eventual total of 500 to 600 dents and 50 to 60 fellows. The total dowment needed for the establishment d maintenance of the new college is 450,000 ($9,660,000). Churchill will nate £25,000 ($70,00). A donation £50,000 ($140,000) has been offered the Calouste Gulbenkian Foundation Lisbon, Portugal, as a "willing tribute the great living Englishman."

British industry is expected to conbute generously to the establishment the college, and Carl J. Gilbert, presint of the Gilette Company of Boston, considering the raising of a fund among ited States corporations with subsidies in Britain or the British Commonwealth.

Copyrights and Russian Translation

United States plans for large-scale anslation into English of Soviet scienfic materials may soon meet another nificant obstacle—Soviet copyright of ese materials. An article in the *New ork Times* by Harry Schwartz reports at organizations engaged in translation Soviet scientific journals in this coun- reported recently that Soviet spokesen have said it was the intention of eir government to adhere this year to e Geneva Universal Copyright Convenon. The United States has ratified this nvention. Such action by the Soviet overnment would give it legal property ghts to Soviet material reaching this untry so that Soviet permission would required for publication or translaion.

One publisher commented that the reat of Soviet adherence to the Geneva onvention was being used to induce merican translators of Soviet scientific aterial to pay royalties to the Soviet overnment. The first such royalty greement, covering 20 Soviet scientific urnals, was made recently between a oviet Government agency and Consultnts Bureau, Inc., of 227 W. 17th St., ew York, N.Y.

There is no legal protection for Soviet roperty rights in literary and related aterial in this country, and there is one for American property of a similar ature in the Soviet Union. If the Soviet nion joins the Geneva Convention it ould radically alter the present econom- s of American translation of Soviet scientific materials.

The chief obstacle to Soviet adherence to the Geneva Convention would be the reciprocal Soviet obligation to honor foreign copyrights. Soviet representatives in this country who have discussed the issue with interested Americans have disclaimed all knowledge of such payment. However, some American authors of scientific books translated and published in the Soviet Union have recently received Soviet payments.

Mouth-to-Mouth Resuscitation

The mouth-to-mouth breathing technique of artificial respiration advocated for children is discussed in four articles in the 17 May issue of the *Journal of the American Medical Association*. The technique, which has been adopted for use on children by the American Red Cross, has been found to be equally effective for adults. Archer S. Gordon and his associates at the University of Illinois College of Medicine have demonstrated through comparative experiments that the mouth-to-mouth technique is "unequivocally superior" to manual techniques in all age groups. Rescuers can maintain mouth-to-mouth breathing for an hour or more without fatigue, even though the victim is twice the size of the rescuer.

Iraqi Scientific Journal

Proceedings of the Iraqi Scientific Societies, volume 1, 1957, has been released. The journal has been established to present original contributions, survey articles, and discussions in mathematics, physics, geophysics, engineering, chemistry, zoology, and botany. Contributions will be published in English, French, or German, with an Arabic résumé for each. Manuscripts should be sent to the editor-in-chief, Prof. Abdul Jabbar Abdullah, Department of Physics, Higher Teachers' College, Baghdad, Iraq.

The Iraqi societies that are supporting the *Proceedings* are the Society of Mathematical and Physical Sciences, the Chemical Society, and the Biological Society. They were formed at Baghdad in 1956.

Grants, Fellowships, and Awards

Arthritis. The Arthritis and Rheumatism Foundation offers predoctoral, postdoctoral, and senior investigatorship awards in the fundamental sciences related to arthritis for work beginning 1 July 1959. Deadline for applications is *31 October 1958*. These awards are intended as fellowships to advance the training of young men and women planning an investigative or teaching career. They are not in the nature of a grants-in-aid in support of research projects. The three types of awards are as follows.

Predoctoral fellowships are limited to students who hold a bachelor's degree. Each applicant studying for an advanced degree must be acceptable to the individual under whom the work will be done. Stipends range from $1500 to $3000 per year, depending upon the family responsibilities of the fellow.

Postdoctoral fellowships are limited to applicants with the degree of doctor of medicine or doctor of philosophy, or the equivalent. Stipends range from $4000 to $6000 per year, also depending upon the family responsibilities of the fellow.

Senior investigator awards are made to candidates holding, or eligible for, a faculty rank such as instructor or assistant professor (or equivalent) and who are sponsored by their institution. Stipends are from $6000 to $7500 per year and are tenable for 5 years.

A sum of $500 will be paid to cover the laboratory expenses of each postdoctoral fellow and senior investigator. An equal sum will be paid to cover the tuition expenses of each predoctoral fellow. For further information and application forms, address the Medical Director, Arthritis and Rheumatism Foundation, 10 Columbus Circle, New York 19, N.Y.

Atomic energy. Sterling Cole, director-general of the International Atomic Energy Agency, announced recently that more than 200 fellowships for training in the peaceful uses of atomic energy are available through the agency. The fellowships' total value is about $1 million. Financed by the agency itself or by individual donations of member governments, the fellowships "correspond to the most urgent needs of less developed countries." Preference will be given to candidates from underdeveloped countries in awarding the fellowships.

The United States is offering 120 fellowships during the next 2 years, while the Soviet Union will accept 25 students for 5 or 6 years of study and 20 students for 3- or 6-month training courses. At present only government-sponsored candidates from member nations of the international atomic agency will be considered for study posts in foreign countries under the fellowship plan.

General. The American Academy of Arts and Sciences invites applications for grants from its Permanent Science Fund. Awards are made in support of research in any field of science whatsoever, in amounts that ordinarily do not exceed $1500. Applications for grants to be made in the early fall should be filed by *1 September* on forms that may be obtained from: The Chairman, Permanent Science Fund Committee, American Academy of Arts and Sciences, 280 Newton St., Brookline 46, Mass.

Special consideration will be given to projects on new frontiers of science; those that lie between, or include, two or more of the classical fields; and those proposed by investigators who may be on the threshold of investigational careers or who are handicapped by inadequate resources and facilities. The committee does not ordinarily approve grants for research the results of which constitute partial fulfillment of requirements for an academic degree.

National Science Board

President Eisenhower has sent nominations to the Senate of the following persons for membership on the National Science Board, governing body of the National Science Foundation: Detlev W. Bronk, president of the National Academy of Sciences and president of the Rockefeller Institute for Medical Research; Lee A. DuBridge, president of California Institute of Technology; T. Keith Glennan, president of Case Institute of Technology; Robert F. Loeb, Bard professor of medicine at the College of Physicians and Surgeons, Columbia University; Kevin McCann, president of Defiance College; Jane A. Russell (Mrs. Alfred E. Wilhelmi), associate professor of biochemistry at Emory University; Paul B. Sears, chairman of the Conservation Program, Yale University; and Ernest H. Volwiler, president and general manager of Abbott Laboratories, North Chicago, Ill.

Bronk, chairman of the board, and Glennan and Loeb were renominated. DuBridge served as a member of the first National Science Board from 1950 to 1954. The term of office of board members is six years.

The outgoing members of the board, whose terms expired on 10 May 1958, are A. A. Potter, dean emeritus of engineering, Purdue University; Sophie D. Aberle, special research director, University of New Mexico; Charles Dollard, president (retired), Carnegie Corporation of New York; and Robert P. Barnes, professor of chemistry, Howard University. A fifth vacancy was created by the death of Gerty T. Cori, former professor of biological chemistry, School of Medicine, Washington University.

News Briefs

Plant Pathology—Problems and Progress 1908–1958 will be published next winter by the American Phytopathological Society, which is celebrating its 50th anniversary. This 1000-page volume of some 60 anniversary symposium papers will be illustrated, bound, and priced at $8.50. Orders should be placed with the American Phytopathological Society, P.O. Drawer 1106, New Haven 4, Conn. Orders are being accepted now; a limited edition will be published.

* * *

A new study by the Public Health Service's National Health Survey shows that as of last August, 42 percent of the American people had not been to the dentist for 3 years or more and that only 36 percent of the people had visited a dentist during the preceding year. The survey also showed that more than 21 million persons, or 13 percent of the population, have lost all their teeth. The *Preliminary Report on Volume of Dental Care, United States, July–September, 1957* may be obtained for 25 cents from the Superintendent of Documents, Government Printing Office, Washington, D.C.

* * *

Archeological investigations have shown that a prehistoric group of Indians descended the Amazon River from the Andes to the Atlantic several centuries before the same trip was made by a European, Orellana, in 1541. Details supporting this conclusion are presented in a recently issued publication of the Smithsonian Institution entitled *Archeological Investigations at the Mouth of the Amazon,* by Betty J. Meggers and Clifford Evans of the Division of Archeology.

* * *

The United States has offered the International Atomic Energy Agency, as a gift, two self-contained and self-propelled mobile radioisotope laboratories designed by the Oak Ridge Institute of Nuclear Studies to provide the tools for basic training in radioisotope handling techniques. Use of the traveling laboratories would enable countries to increase the number of personnel trained in radioisotope utilization without the expense of sending all trainees abroad. The cost of the two mobile laboratories is estimated to be $85,000.

* * *

On 26 May President Eisenhower dedicated the Shippingport Atomic Power Station, perhaps the first full-scale central station atomic power plant devoted exclusively to civilian uses. The President, from the White House, used a neutron source to send an impulse to Shippingport that opened the main turbine throttle valve at Shippingport and sent a flow of electricity into homes, stores, and industrial plants of the Pittsburgh area. The initial core of the reactor is capable of furnishing sufficient heat for the production of 60,000 net kilowatts of electricity.

* * *

The third annual volume of the *Bibliography of Medical Reviews* has been announced. It is arranged by subject, with a separate author index, and contains approximately 2900 references to review articles in clinical and experimental medicine and allied fields. Most of the articles appeared in 1957. Copies may be obtained from the Superintendent of Documents, U.S. Government Printing Office, Washington 25, D.C.

* * *

Australia's first nuclear reactor was set in operation on 18 April by the prime minister, R. G. Menzies, when he formally opened the Atomic Energy Research Establishment at Lucas Heights, near Sydney.

Proposed Legislation

Of the many bills introduced in Congress, some have a special relevance to science and education. A list of such bills recently introduced follows:

HR 11985. Establish within the National Science Foundation a National Scientific and Technical Information Service for the collection and dissemination of information in fields of science and technology. Celler (D-N.Y.). House Interstate and Foreign Commerce.

S 3632. Amend P.L. 85-162 to increase authorization for appropriations to the Atomic Energy Commission in accordance with section 261 of Atomic Energy Act of 1954, as amended. Anderson (D-N.M.). Joint Committee on Atomic Energy.

S 3619. Establish a National Wilderness Preservation System for the permanent good of the whole people, to provide for the protection and administration of the areas within this system by existing federal agencies and for gathering and dissemination of information to increase the knowledge and appreciation of wilderness for its appropriate use and enjoyment by the people, to establish a National Wilderness Preservation Council. Neuberger (D-Ore.). Senate Interior and Insular Affairs.

HR 12127. Provide for a program of federal grants-in-aid to assist the states to establish and maintain science and technological centers providing adequate facilities for advanced education and research in certain fields of science and engineering. Matthews (D-Fla.). House Education and Labor.

HR 12051. Provide for establishment of Bureau of Older Persons within the Department of Health, Education, and Welfare; authorize federal grants to assist in development and operation of studies and projects to help older persons. Libonati (D-Ill.). House Education and Labor.

S 3642. Provide for accelerated development of secondary school education in natural sciences in the several states and territories. Yarborough (D-Texas). Senate Labor and Public Welfare.

...tists in the News

...e following have been elected for... members of the Royal Society, Eng... ANDRE LWOFF (Paris), distin... ...ed for his work on the morphology ...nutritional requirements of protozoa ...for his researches on lysogenic bac... ...and bacteriophage; NIKOLAI ...ENOV (Moscow), distinguished ...his work in chemical kinetics, espe... ...for his pioneer work on chain re...ms; GEORGE GAYLORD SIMP... ...(New York), distinguished for his ...ributions to paleontology and evolu... ...and ARTHUR STOLL (Basle), ...guished for his work on the chem... ...of natural products.

...P. MILES, JR., of Alabama Poly... ...ic Institute is completing a year as ...ing associate professor at the Insti... ...for Fluid Dynamics and Applied ...hematics, University of Maryland, ...re he has been doing research in par... ...differential equations, supported by ...ntract granted under the Air Force ...batical Leave Plan. Miles will serve ...summer as a professonial assistant in ...Division of Scientific Personnel, Na... ...al Science Foundation and will join ...staff of Florida State University, Tal... ...ssee, as an associate professor of ...hematics this fall.

...AMES C. THOMSON, formerly ...dical consultant for the World Health ...anization in Iran and nutrition con... ...ant for both WHO and FAO in ...istan and Turkey, has been appointed ...fessor of nutrition in the Yonsei Uni... ...ity Medical School and the Sever... ...e Medical Center in Seoul, Korea.

...WILLARD F. LIBBY, commissioner ...the U.S. Atomic Energy Commission, ...received the 1958 Willard Gibbs ...dal of the American Chemical So... ...y's Chicago Section. The medal, one ...the highest honors in American chem... ...y, is conferred annually upon an out... ...nding scientist, selected by a national ...y, "who, because of his eminent work ...and original contributions to, pure ...d applied chemistry, is deemed worthy ...special recognition." Libby was recog... ...ed for his development of the atomic ...ne-clock method of determining geo... ...gical age and for his discovery that ...ium—a radioactive form of hydrogen ...can be used to trace meteorological ...d geophysical processes.

...J. BRACHET, professor at the Free ...iversity of Brussels (Belgium), has re... ...ived the 1957 Leopold Mayer Prize of ...e Société de Chimie Biologique ...rance). This is the third award of the ...0,000-franc prize, which is given for ...search in nucleoproteins. The prize was established in 1954 for presentation annually for 6 years. It is administered by the council of the Société de Chimie Biologique, which has headquarters at 4, Avenue de l'Observatoire, Paris. VIe.

K. E. FIELDS, general manager of the Atomic Energy Commission, will resign on 1 July. Fields, a graduate of the U.S. Military Academy, was assigned in November 1945 as assistant to the commanding general, Manhattan Project. In August 1951 he was appointed director of military application for the Atomic Energy Commission, and held that post until 1955 when he became general manager. Fields' academic degrees include Bachelor of Science from the U.S. Military Academy, and Master of Science from the Massachusetts Institute of Technology, 1938, and from the Graduate School of Engineering, Harvard University, 1939.

CHARLES FELDMAN and ELIAS BURSTEIN, both of the Naval Research Laboratory, have earned the annual science awards of the U.S. Naval Research Laboratory Branch of the Scientific Research Society of America. Feldman received the society's Applied Science Award for his discovery and practical development of transparent luminescent films, and Burstein received the Basic Science Award for his pioneering work in using infrared radiation to investigate the fundamental properties of semiconductors.

AUGUSTUS H. FOX, professor of mathematics and chairman-elect of the department of mathematics at Union College in Schenectady, N.Y., has been elected chairman of the Federation of American Scientists, which has headquarters in Washington, D.C. He succeeds PAUL M. DOTY of Harvard University. Vice-chairman for 1958–59 is WALTER SELOVE, professor of physics at the University of Pennsylvania.

ERWIN L. JUNGHERR, head of the department of animal diseases at Connecticut University Agricultural College and an international specialist in poultry diseases, has received the $1000 Distinguished Service Research Award of the Animal Health Institute.

W. WILBUR ACKERMANN, associate professor in the Virus Laboratory, department of epidemiology, University of Michigan School of Public Health, received the $1000 Eli Lilly Award for outstanding research at the recent annual meeting of the Society of American Bacteriologists. He was honored for his biochemical studies on the reproduction of the viruses of influenza and poliomyelitis.

WALTER P. TAYLOR, on leave from the Claremont Graduate School and La Verne College, has been serving during the present school year as visiting professor of zoology at Southern Illinois University. He will go to Europe this summer to attend international meetings. Before returning to California, he will spend a year studying the geography, ecology, and conservation of the Mediterranean countries and Europe, including the Scandinavian peninsula.

Recent Deaths

GEORGE J. ELTZ, Avon, N.J.; 65; director of the engineering facilities division of the Army's signal research and development laboratory at Fort Monmouth; 15 May.

LUCY E. GUELEZIAN, Philadelphia, Pa.; 79; former physician at Woman's Hospital and a staff member of the University of Pennsylvania Hospital; 11 May.

MAXWELL HERMAN, Philadelphia, Pa.; 69; retired in 1954 as chief ophthalmologist at the Einstein Medical Center; staff-member of the Wills Eye and Jefferson Hospitals; 16 May.

HAROLD W. JONES, Orlando, Fla.; 81; retired colonel, U.S. Army, and director of the Army Medical Library from 1936 to 1945; 5 Apr.

VSEVOLOD N. KRIVOBOK, New York, N.Y.; 65; supervisor of the stainless steel and heat-resistant alloy section of the International Nickel Company's Development and Research Division; professor of metallurgy at Carnegie Institute of Technology in Pittsburgh from 1924 to 1940; 17 May.

EUGENE F. McDONALD, JR., Chicago, Ill.; 68; founder and chairman of the Zenith Radio Corporation; developed many devices now standard in radio, television, and radar work; 15 May.

LAWRENCE T. POST, St. Louis, Mo.; 70; head of the department of ophthalmology at Washington University; past president of the American Academy of Ophthalmology and Otolaryngology; 13 May.

MAURICE B. RAPPAPORT, Brookline, Mass.; 51; senior research engineer in charge of biophysical research and development at the Sanborn Company, Waltham, Mass.; specialist in medical electronics; 7 May.

EUGENE S. SAGI, New York, N.Y.; 66; pathologist and specialist in cancer research.

FRANK B. SANBORN, Cambridge, Mass.; 93; engineer and former professor of civil engineering at the Tufts University Engineering School; invented several measuring devices; author of *Basal Metabolism* and a college textbook on mechanics; 16 May.

Book Reviews

Russia, the Atom and the West. George F. Kennan. Harper, New York, 1958. 116 pp. $2.50.

This slender volume, like Machiavelli's *Prince,* has a significance out of all proportion to its size. It has been more widely debated, denounced, defended, or deplored than any comparable postwar book on foreign affairs. The extraordinary attention which the book has received is due, no doubt, in part to the reputation of its author. For George Kennan writes as one having authority. He is not only a distinguished historian and student of foreign policy but has been a leading practitioner of the science and art of international diplomacy. A long career in the foreign service, including assignments as American ambassador to the U.S.S.R. and counselor of the Department of State and director of its Policy Planning Staff, has given him a knowledge and perspective not commonly found among the movers and shakers of our contemporary world. Moreover, the essays deal with a subject matter—Russia, the atom, and the West—toward which no thoughtful citizen of the free world can be indifferent.

The five Reith lectures, delivered over BBC—plus a sixth essay, on Anglo-American relations—focus attention on some of the most explosive problems of contemporary world politics. Central to all of these is, of course, the Russian challenge. The spectacular achievement of the Soviet Union in science and technology, of which the sputnik is but a symbol, her growing military might, and her skill at diplomatic maneuver and political propaganda have aroused fear and misgiving of panic proportions in the free world. Our own response has been characterized by frantic self-depreciation of our own heroic achievements in nearly every field of endeavor; "crash" programs for the training of scientists and engineers; radical shifts in military procurement policies, from strategic bombers to guided missiles; and widespread talk about putting aside the normal goals of a healthy civilian economy for the Spartan objectives of a garrison state. In a word, we are being told that our salvation in education, national defense, economic policy, and diplomacy—lies in becoming as much like the Soviet Union as is possible within the limits of our indulgent, loose-jointed democratic culture.

Kennan takes sharp issue with this point of view, which, he says, has caused us to "believe that every Soviet gain is automatically our loss and to see our salvation as dependent on our ability to outpace Russia in every single phase of her economic progress." This belief on our part has enabled the Soviet Government to exploit her own achievements for political and propaganda purposes. "It has endeavored at every turn to present itself as participating in an all-out competition with the Western countries for industrial growth and then to interpret every element of its economic progress as a triumph for its own system of economy and a defeat for the Western World."

A more objective and realistic appraisal of Russian economic development would see it not as a product of some *mystique* inherent in communism or a totalitarian society but in terms of a "large and vigorous population, rich in talents of every sort . . . [and] a territory liberally endowed with resources which permit successful industrialization everywhere." Moreover, the rate of Russian economic growth must be interpreted in terms of the base from which that growth began. Given the human and natural resources of the Soviet Union "and the spirit of the modern age," rapid industrialization was inevitable. Nor, says Kennan, should we view this with alarm; "It will be a happy day for everyone when they too have solved their problems of production and can join us in grappling with some of the deeper more subtle, and more significant problems that lie at the end, rather than at the beginning, of the economic rainbow." If the United States is to meet the Soviet challenge, we had better apply ourselves "to our own American failings . . . to the racial problem, to the conditions in our big cities, to the problems of education. . . ." In final analysis, he says, "Whether we win against the Russians is primarily a question of whether we win against ourselves."

Most men of good will in America would agree with this analysis. But would the rulers of Soviet Russia? Our fear arises not from Russia's rapid industrialization or her spectacular achievement in science and technology but from the possible uses to which these may be put. It arises, in short, from the threat of Soviet military might coupled with an ideology that sees international politics almost solely in terms of a friend-enemy relationship. So long as Russia's leaders, obsessed with paranoid fears of "capitalist encroachment and aggression," see Russia's own welfare and security as dependent upon the triumph of world communism, even men of good will in the West will be unwilling to accept Kennan's apparent quietism.

When this form of political paranoia is accompanied by a vast military establishment, including a large stockpile of atomic weapons, and by a demonstrated willingness to employ not only propaganda and subversion but violence as instruments of policy, who can say our fears are groundless?

Kennan does not deny all this but rather demurs to it. Admitting Soviet obsessions and paranoia, admitting too the deliberate alienation or isolation of the Russian people from the international world of reality, he argues for a policy calculated to mitigate, if not to cure, these ills. Now that substantial atomic parity exists between Russia and the United States, Kennan believes that the fearful possibility and the catastrophic character of modern nuclear warfare can be a force for peace—if only through stalemate. Nuclear weapons have become such effective instruments of mass annihilation, and the certainty of instant retaliation has become so generally recognized, that neither of the great powers dares risk an all-out war, although the possibility of such a calamity must not be ruled out. Short of all-out war, however, the conflict between Russia and the West will go on, with economic and ideological weapons and here and there outbursts of violence to achieve local or limited objectives. Kennan does not, however, share Henry Kissinger's belief in the possibility—not to say desirability—of so-called limited wars being carried on by domesticated or "tactical" atomic weapons. "Can we really suppose," he asks, "that poor old Europe, so deeply and insidiously weakened by the ulterior effects of the two previous wars of this century, could stand another and even more horrible ordeal of this nature?"

The agony of this generation is not to be resolved by limited wars or by all-out war but rather by a progressive reduction of international tension to a point where constructive and realistic diplomacy can again play its proper role in the politics of nations. To this end Kennan proposes a policy of military disengagement in Europe and of friendly interest and watchful waiting in Africa and

Asia. "I would only say," he writes, "that it seems far more desirable on principle to get the Soviet forces out of Central and Eastern Europe than to cultivate a new Germany for the purpose of opposing them while they remain there." And if we are to get the Soviet forces out of Central and Eastern Europe, we shall have to withdraw our own forces from most of Europe. Once this mutual withdrawal has been agreed upon, a number of things become possible. Unification of Germany on the basis of free elections will become feasible—provided a united Germany is neutralized as between NATO and the U.S.S.R. Only when Soviet troops are withdrawn from the present Russian satellites such as Poland, Hungary, and Czechoslovakia can there be any hope for the development of free democratic governments in those countries. But, as in the case of Germany, a condition of their freedom will be a policy of neutrality as between Russia and the West. Neutrality and demilitarization (save only for paramilitary police) would be a small price to pay for their independence and the progressive relaxation of international tension in Eastern Europe.

It is on some such terms that Kennan would attempt to mitigate, if not to cure, the political paranoia that afflicts the government and people of the Soviet Union, and to free Europe from the paralyzing fear of nuclear war.

There are calculated risks in this, as in any policy. What if the Russians—who are not above lying and breaking agreements when it suits their purpose—were to "move in" on Europe once our own forces had moved out? Would this not leave us with no other alternative than all-out nuclear war on the Soviet Union, and thus invite the very catastrophe we seek most to avoid? To these questions Kennan offers two answers. "We must get over this obsession," he says, "that the Russians are yearning to attack and occupy Western Europe." The Soviet threat, he argues, is primarily political, not military, and when the defense of Europe becomes a problem for Europeans without "the armed forces of the United States and Britain," the people of Europe will show more initiative, energy, and imagination to that end than they now exhibit. Their major problem would then be one of "internal health and discipline . . . to prevent the conquest and subjugation of their national life by unscrupulous and foreign-inspired minorities in their midst." But would not the withdrawal of American and British forces from Europe give aid and comfort to the already powerful Communist minorities in France and Italy, for example? To prevent this, Kennan urges the strengthening of Europe's own defense forces and, more especially, establishment of paramilitary or territorial militia, trained to put down internal uprising and subversion. "I can give personal assurance," writes Kennan, "that any country [that does this] . . . will have little need of foreign garrisons to assure its immunity from Soviet attack."

It is this note that Dean Acheson has labeled "messianism" in Kennan, and one is led to wonder whether, on balance, Europe can more safely rely on Kennan's "personal assurance" than on the armed forces of NATO, including the United States and Britain.

There is much more in this book that deserves comment. Kennan's views on foreign aid are a strange mixture of neo-isolationism, moral indignation, and faith in a kind of Machiavellian power politics. "I . . . reject the suggestion," he says, "that our generation in the West has some sort of cosmic guilt or obligation vis-à-vis the underdeveloped parts of the world. The fact that certain portions of the globe were developed sooner than others is one for which I, as an American of this day, cannot accept the faintest moral responsibility." If we are told that without our aid this country or that will go Communist, Kennan would say, "Very well, then go. American interest will suffer, but yours will suffer first." Besides, he says, a "sizable portion of mankind has more respect for power and success than it has for principle." Just what, in the light of all this, he means when he says, "If we are to help each other in this world, we must start with a clean slate," I am at a loss to know. As though in politics or anything else one *ever* starts "with a clean slate."

Whatever one may think of Kennan's specifics, his book represents a refreshingly high level of argument and analysis in the most difficult science in the world—the science of politics. Moreover, as James Reston of the *New York Times* has observed, Kennan can write. "Much of the political debate in this country," Reston says, "sounds like the droning of two old bagpipes." Not so in Kennan's case. Learning and wisdom are combined with a brilliant prose style, as for example, in his comment on the changing technological realities of present arms competition and their consequences. "Are we," he asks, "to flee like haunted creatures from one defensive device to another, each more costly and humiliating than the one before, cowering underground one day, breaking up our cities the next, attempting to surround ourselves with elaborate electronic shields on the third, concerned only to prolong the length of our lives while sacrificing all the values for which it might be worthwhile to live at all?"

If this is to be our future, he would say, "let us divest ourselves of this weapon altogether; let us stake our safety on God's grace and our own good consciences, and on that measure of common sense and humanity which even our adversaries possess; but then let us at least walk like men, with our heads up so long as we are permitted to walk at all."

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Human Protein Requirements and Their Fulfilment in Practice. Proceedings of a Conference in Princeton, [N.J.] United States (1955). Sponsored jointly by the Food and Agriculture Organization of the United Nations; the World Health Organization; the Josiah Macy Jr. Foundation. J. C. Waterlow and Joan M. L. Stephen, Eds. Wright, Bristol, England. 193 pp. $2.

The rapid increase of world population has led to the fear that the human race may outrun its food supply. Although there exists no danger that energy food may become short in the immediate future, as far as protective foodstuffs are concerned, many areas of the world are suffering already from serious deficiencies which are particularly detrimental from the standpoint of the healthy development of children. While the more spectacular vitamin deficiencies have been given proper attention, it is only lately that the need for a well-balanced protein intake containing adequate amounts of all essential amino acids has been emphasized. Thanks to the endeavors of numerous scientists and such international organizations as the Food and Agriculture Organization of the United Nations, the World Health Organization, and the United Nations International Children's Emergency Fund, we have gone a long way not only in recognizing the requirements for proteins of a certain amino acid composition in the various stages of human development but also in meeting these needs cheaply and economically in areas of malnutrition.

In this respect the Princeton conference of 1955, arranged by the above-mentioned agencies with the assistance of the Josiah Macy Jr. Foundation, was a great step forward, inasmuch as it not only arrived at quantitative determination of protein requirements but also removed the specter of protein starvation due to inadequate supply of animal products. In this respect results obtained in feeding mixtures of certain seed proteins, such as soybean, peanut, cottonseed, and sesame cake flour, are very promising, especially in children's diets. Since many of these products can be obtained as by-

products of oil extraction and are presently used for animal nutrition, their direct use as food supplement for human beings in areas of shortage should not be too difficult, or economically prohibitive.

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The American Idea of Mission. Edward McNall Burns. Rutgers University Press, New Brunswick, N.J., 1957. xii + 385 pp. Illus. $9.

America is not alone in possessing a sense of national destiny, but with us this feeling has been especially acute and pervasive. This sense of mission, according to the author, centers around a number of convictions: that America has developed more completely than have other nations the principle of liberty; that we are the greatest exemplar of human equality; that ours is the most democratic of all governments; that we are more imbued with a love of peace than are the quarreling peoples of other parts of the earth; that we can lead the world to greater happiness by the example of a high standard of living.

The author supports his conclusions from the works of a host of political leaders, historians, essayists, and other writers. Among these may be mentioned James Wilson, Madison, Hamilton, Franklin, Calhoun, Lincoln, Theodore Roosevelt, Woodrow Wilson, Harry Truman, Bancroft, Fiske, Beard, Emerson, Edward Everett, Wendell Phillips, Henry George, David Starr Jordan, Albert J. Beveridge, and Walter Lippmann. Despite the differences in the periods in which these men were active and the variety of their respective natures, there runs throughout their utterances a note of optimism, sometimes mystical in tone, about American character and institutions that promises well for the world.

It is inevitable that the historian of our ideals should see discrepancies between the conception we have formed of ourselves and the reality of our attainment—that courts have upheld laws that restricted liberty; that racism has conflicted with the doctrine of human equality; that forms of censorship have denied the freedom that democracy implies; that glorification of victory is not consistent with a hatred of war; that many families with low incomes cannot enjoy the living standards of which we boast. Even our conception of ourselves may be imperfect and, as the author points out, where we credit our superiority to "initiative, independence, aggressiveness, perseverance, industry, frugality, and enterprise," such New Testament qualities as generosity, humanity, tolerance, and justice are seldom included in the boast.

In making clear the weaknesses that have appeared in our estimate of ourselves and of our destiny, the book seems sometimes to undervalue the idealistic element in American thought and action. A nation that threw its weight into winning a great war from which it claimed no material reward, other than some indefinite claims on the island of Yap, may have entered that war in enthusiastic innocence and deceived itself cruelly as to what it was accomplishing. But certainly we had raised ourselves above the common level of victors, and this record of altruism is not cancelled out by recalling what we did to the Indians.

If we admit that our conception of ourselves often has been naive, it has nevertheless been an ideal, and the ideal is the first step toward attainment. This the author may have in mind when he says that our sense of mission runs like a golden thread through most of our history and, "purged of its dross of conceit and illusion . . . remains one of the noblest expressions of idealism that any nation has embraced."

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The Biological Action of Growth Substances. Symposia of the Society for Experimental Biology, No. XI. H. K. Porter, Ed. Academic Press, New York; Cambridge University Press, London, 1957. viii + 344 pp. $9.50.

Symposia on animal hormones—for example, the Annual Laurentian Conference—are frequently held, and symposia on plant hormones take place occasionally. The present symposium differs from others in being almost equally divided between plants and animals. Although it is unfortunately true that the speakers in zoology virtually never refer to work with plants, and vice versa, still the juxtaposition is stimulating to the reader and must have been more so to the participants in the symposium. A few topics are dealt with by both groups. The growth-promoting effects of antibiotics, discussed by J. W. G. Porter for farm animals and touched on for some plants by P. W. Brian, are still not fully explained, although Porter makes a strong case for an antibacterial effect, exerted on Clostridia causing subclinical intestinal infections, which, in the normal animal, would reduce growth below the optimum. The development of tumors from normal tissue, described by A. Braun for plant tissue cultures and by G. Klein and E. Klein for animals, shows marked parallelism between the two kingdoms, for both articles present the change as essentially the development of autonomy, or independence from specific growth stimulators and inhibitors. Braun gives evidence for the activation or unblocking, in crown gall, of systems synthesizing four different growth substances, including auxin and a cell-division factor, while G. Klein and E. Klein envisage two basically different types of mechanism, one depending on the selection of variant cells, the other on modifications caused directly by a factor in the host environment.

P. W. Brian discusses the causes of overgrowth in plant diseases, comparing in particular the actions of auxin and gibberellin; he shows proper caution in ascribing the observed overgrowth to specific factors, in the absence of rigid proof, but he makes a common mistake when he compares the ability of pathogens to produce galls with their ability to form auxin in pure culture. It is of course the ability to form auxin in the host tissue that is critical, since culture media may not duplicate the nutrition supplied by the host tissue. Gregory and Veale's paper on "Apical dominance in plants" (inhibition of lateral buds by the terminal) exemplifies the bankruptcy of ideas that has beset this subject in recent years; having chosen a plant in which the influence of the terminal bud —that is, the hormonal factor—is evidently weak, these authors conclude that the main determinant for lateral bud growth is nutrition, especially nitrogen supply. It is not surprising that if growth is not being strongly inhibited it will be limited by some nutritive factor. The effect of auxin in inhibiting lateral bud growth they ascribe to its interference with the formation of provascular strands, in spite of the fact that several workers (Camus and Wetmore, with tissue cultures, and Jacobs, with whole plants) have shown that auxin strongly promotes this process. A much more satisfying approach to the old problem of apical dominance develops from the work on the interrelations between auxin and kinetin in tissue culture, discussed by F. Skoog and C. O. Miller. Indeed, recent work in my laboratory demonstrates that applied kinetin can largely offset typical lateral bud inhibition exerted by the terminal bud, while the similar inhibition exerted by externally applied auxin can be overcome completely.

Two papers, by J. D. Biggers and coworkers and by E. Wolff, respectively, are largely concerned with the complex nutritive requirements of animal tissue cultures, and a useful compilation of data is included. Two others, by L. Brauner and A. R. Schrank, deal with tropisms in plants, the former with the light gradient necessary for phototropism and the latter with bioelectrical aspects both of tropisms and of curvatures caused by auxin. A thoughtful and analytical treatment of the problems involved in

...e local formation of new tissue in the ...ult mammal, such as liver regenera... ...on, is contributed by M. Abercrombie. The mode of action of growth sub... ...tances and hormones at the molecular ...evel is curiously neglected in this vol... ...me, although action at higher levels is ...ften introduced. Thus, B. L. Baker and ... C. Pliske lay stress on the regulation ...f enzyme secretion by the pituitary ...hrough its action on the zymogenic cells. ... J. Randle, in discussing the pituitary ...rowth hormone, draws attention to its ...rotein anabolic action and its insulin-...ke hypoglycemic effect and summarizes ...e evidence that the former may be ...ediated by way of insulin itself. It is ...emarkable, however, that, as A. Jost ...nd L. Picon show, the fetus of rat or ...abbit can develop at nearly normal ...tes in the complete absence of pitui... ...ary hormones, either from their own or ...rom maternal sources. Perhaps insulin ...r some other nonpituitary hormone ...lays a controlling part here. V. B. Wig...lesworth discusses the action of growth ...ormones in insects and is inclined to ...scribe the action both of the juvenile ...ormone and of ecdysone to their "regu...ation of permeability relations within ...e cells," whereby enzymes and coen...ymes or substrates can be brought to...ether. H. Burström, in a discussion of ...oot growth, visualizes auxin as acting ...irectly on the cell wall of the root; on ...e one hand it increases plastic stretch...ng but, on the other, it inhibits the sub...equent active laying down of wall ma...erial and thus inhibits elongation as a ...hole. Lastly, F. Skoog and C. O. Miller ...conclude that there is now a gradual ...blurring of the distinctions between "hor...mones, metabolites, and structural units," ...s these "grade into each other in in...egrated biosynthetic systems which func...ion in all types of growth."

As with most symposia, the treatment ...s extensive but not systematic, and the ...ook has no index. But this is a thought...provoking collection.

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...arvard University

...urity Control by Thermal Analysis. Proceedings of the International Symposium on Purity Control by Thermal Analysis, Amsterdam, 1957. M. W. Smit, Ed. Elsevier, Amsterdam, 1957 (order from Van Nostrand, Princeton, N.J.). xii + 182 pp.

This is a collection of papers presented ...t an international symposium held in ...msterdam in April 1957, under the ...ponsorship of the International Union ...f Pure and Applied Chemistry, acting ...hrough its Commission on Physico-...hemical Data and Standards. The same material, exclusive of the seven-page digest of oral discussion, also appeared in *Anal. Chim. Acta* **17**, No. 1 (1957).

Quantitative application of the freezing (melting) temperature of a substance, and especially of the variation in this temperature as a function of the relative amounts in the solid and liquid phases, is a relatively new technique for evaluating the purity of chemical substances. From its beginnings, a little more than twenty years ago, the procedure has enjoyed increasing acceptance and has attained considerable industrial importance. With its increasing use has come also an increasing awareness of the possibility of errors in measurements and of the limitations of understanding of the phenomena involved.

The Amsterdam symposium was designed to bring together for discussion as many as possible of the scientists actively concerned and to invite a number of them to present formal papers. The volume contains 16 papers and a brief digest of oral discussion. Four of the authors are from Great Britain, seven are from the United States, three are from the Netherlands, one is from Germany, one is from Poland. The papers cover a wide range of experimental methods, both thermometric and calorimetric, for constructing freezing and melting curves. Three deal with instrumentation. Taken together these papers constitute the most authoritative source of information available on the cryometric evaluation of purity.

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Flora Hawaiiensis. Book 5, *The New Illustrated Flora of the Hawaiian Islands.* Otto Degener. The author, Waialua, Oahu, Hawaii, 1957. $5.

In this fifth part of a loose-leaf flora are 217 sheets and a temporary index, with page indications to show where each sheet fits into the complete work, publication of which began in 1946. As in previous sections, the size of the type is varied from page to page, to fit the material to the space allotted and give prominence to the fine line drawings of each species.

The "Flora" of the title refers only to tracheophytes, but the author gives family identifications and keys to genera and species, technical descriptions of individual varieties, and a wealth of delightful information as well as data on distribution. The current "book" includes, among others, *Monstera deliciosa,* the banana family, *Casuarina equisetifolia,* ramie, sandalwood, a Hawaiian sundew (possibly introduced by Pacific golden plovers), a long key to local members of the pea family, papaya, and frangipani. Many of the introduced species have developed racial differences, but the conflict between ancient and recent additions to the islands' plant life is evident throughout the descriptions.

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New Books

Principles of Chemistry. Donald C. Gregg. Allyn and Bacon, Boston, 1958. 620 pp. $6.50.

An Introduction to the Dynamics of Airplanes. H. Norman Abramson. Ronald, New York, 1958. 233 pp. $4.50.

Physique Electronique des Gaz et des Solides. Michel Bayet. Masson, Paris, 1958. 246 pp. F. 4900.

Obok. A study of social structure in Eurasia. Viking Publ. in Anthropology, 25. Elizabeth E. Bacon. Wenner-Gren Foundation for Anthropological Research, New York, 1958 (order from Executive Secretary, American Anthropological Assoc.). 250 pp. Paper, $4.

A Dictionary of Mountaineering. Definitions, names, and terms and their explanations, used by English-speaking mountaineers particularly in Britain and on the continent. R. G. Collomb. Philosophical Library, New York, 1958. 175 pp. $6.

Fat Consumption and Coronary Disease. The evolutionary answer to this problem. T. L. Cleave. Philosophical Library, New York, 1957. 40 pp. $2.50.

The Growth of Logical Thinking from Childhood to Adolescence. An essay on the construction of formal operational structures. Barbel Inhelder and Jean Piaget. Translated by Anne Parsons and Stanley Milgram. Basic Books, New York, 1958. 382 pp. $6.75.

Agricola on Metals. Bern Dibner. Burndy Library, Norwalk, Conn., 1958. 128 pp.

Pharmacology in Medicine. A collaborative textbook. Victor A. Drill, Ed. McGraw-Hill, New York, ed. 2, 1958. 1284 pp. $19.50.

Of Stars and Men. The human response to an expanding universe. Harlow Shapley. Beacon Press, Boston, 1958. 164 pp. $3.50.

Nuclear Structure. Leonard Eisenbud and Eugene P. Wigner. Princeton Univ. Press, Princeton, N.J., 1958. 135 pp. $4.

The Relation of Psychiatry to Pharmacology. Abraham Wikler. Williams & Wilkins (for American Soc. for Pharmacology and Experimental Therapeutics), Baltimore, Md., 1957. 330 pp. $4.

Proceedings of the Second International Congress of Surface Activity. vol. I, *Gas/Liquid and Liquid/Liquid Interface,* 521 pp.; vol. II, *Solid/Gas Interface,* 348 pp.; vol. III, *Electron Phenomena and Solid/Liquid Interface,* 621 pp.; vol. IV, *Solid/Liquid Interface (Washings, Etc.) and Cell/Water Interface,* 352 pp. J. H. Schulman, Ed. Academic Press, New York; Butterworths, London, 1957. $50 per set.

Dynamics of Behavior. Robert S. Woodworth. Holt, New York, 1958. 413 pp. $5.

Reports

Immunochemical Studies with Tomato Leaf Proteins

Crude protein preparations from plant tissues frequently give nonspecific precipitation reactions with rabbit serum, which complicate their study by immunochemical methods. Such nonspecific reactions have been avoided by purification of the crude protein preparations or by absorption techniques. Both these procedures remove some of the plant proteins which one may want to study.

In an investigation of the effect of *Fusarium* infection on leaf proteins of tomato, a nonspecific reaction with normal rabbit serum was observed. We wanted to study as many of the plant proteins as possible, hence purification or absorption of the crude protein extracts was not desirable. It seemed that this nonspecific interaction involved serum proteins other than the immune globulins; therefore the fractionation of serum was examined as a possible alternative method of eliminating the nonspecific precipitation reactions. The antigen preparation was obtained from leaves of the tomato variety Bonny Best. The leaves were frozen in liquid air, ground to a fine powder with carbon dioxide, and freeze-dried. Two and one-half grams of the dry powder were extracted at 4°C for 24 hours in a glass tube revolving on a dialysis wheel with 430 ml of 0.05*M* phosphate buffer (*p*H 7.4) containing Merthiolate (1:5000). The cold protein extract was filtered through glass wool and centrifuged in the "Spinco" ultracentrifuge (at 2500 rev/min for 1 hour, with a No. 30 rotor). The crude extract, which contained 121 μg of protein nitrogen per milliliter, was concentrated to approximately 50 ml by pervaporation by means of dialysis (*1*). This concentrated protein extract, containing 588 μg of protein N per milliliter, was brown in color due to the formation of melanin. The melanin was not removed by dialysis; it was prevented from forming in later experiments by addition of ascorbic acid to the extraction medium. The concentrated protein solution [in 0.05*M* phosphate buffer, containing Merthiolate (1:5000)] was stored in the Deepfreeze.

For the immunization of the rabbits, 32 ml of the concentrated protein solution was mixed with 16 ml of a 1-percent aluminum ammonium sulfate solution as an adjuvant [in 0.9-percent saline, containing Merthiolate (1:5000)]. This mixture was adjusted to *p*H 7.0 with sodium hydroxide and diluted to 120 ml to give an antigen solution with 160 μg of protein N per milliliter (approximately 0.1 percent protein).

Before the immunization, a sample of normal serum was obtained from each rabbit. For the immunization, 0.5 ml of antigen solution was injected into each rabbit on four days of the first week, 1.0 ml of antigen solution on each of four days of the second week, 2 ml of antigen solution on four days of the third week, and 3 ml of antigen solution on each of four days of the fourth day. The blood was collected seven days after the last injection, and the serum was stored in the Deepfreeze (*2*).

Part of the sera was subjected to ammonium sulfate fractionation (*2, 3*); another aliquot was used for the preparation of the γ-globulin fractions by alcohol precipitation (*4*). Ring tests were conducted with the antigen and the rabbit serum preparations after various degrees of purification. The antigen solution gave positive ring tests with the homologous antiserum and with normal rabbit serum. This indicated that the whole unfractionated normal serum or antiserum gave a nonspecific precipitation with the crude plant protein antigen. This nonspecific precipitation reaction also occurred with both γ-globulin fractions prepared by ammonium sulfate precipitation. When the antigen was tested with the γ-globulin fractions obtained by alcohol fractionation (*4*), no precipitation reaction could be detected with the γ-globulin preparation from normal serum. There was a strong positive ring test between the antigen and the γ-globulin fraction from the homologous antiserum. The ring tests were repeated with six independently isolated preparations of γ-globulin fractions. Each time the same results were obtained, indicating that the nonspecific precipitation reaction can be avoided if the rabbit serum is sufficiently purified and that this can be achieved by alcohol fractionation of the sera.

To investigate further the specific precipitation reaction between the antigen and the γ-globulins obtained by alcohol fractionation, experiments were carried out to determine the equivalence point and the amount of nitrogen in the precipitate formed by the interaction of the γ-globulin fraction with increasing large amounts of antigen. Increasing large amounts of the antigen in 0.5 ml of 0.05*M* phosphate buffer (*p*H 7.4) were added to 0.5-milliliter aliquots of the γ-globulin preparation. After storage for 48 hours under refrigeration, followed by centrifugation (in an International centrifuge, for 30 minutes at 3000 rev/min, with rotor No. 845, refrigerated), the supernatants were decanted and used for the determination of the equivalence point. The precipitates were washed with 5 ml of cold saline, centrifuged as above, decanted, and drained. The washed precipitates were used for the nitrogen determinations (*5*). The point of equivalence for 0.5 ml of γ-globulin preparation was reached when between 80 and 160 μg of antigen had been added. The results of the nitrogen determinations on the precipitates are given in Fig. 1.

The curve in Fig. 1 shows a region of antibody excess with little precipitate formation and an equivalence zone of maximum precipitation, followed by a region of antigen excess in which little or no precipitate is formed. Maximum precipitation, as judged by the amount of precipitated nitrogen, was observed at a level of approximately 160 μg of antigen per sample. The highest amount of antibody N precipitated occurred at a level of 80 μg of antigen per sample; this indicated that the antibody titer of the γ-globulin fraction used was approximately 11 μg of antibody N per milliliter.

The results of this investigation show that tomato leaf proteins give nonspecific precipitates with rabbit serum. Specific precipitation reactions between tomato leaf proteins and the homologous rabbit antiserum can be obtained if the γ-globulin fraction of the antiserum is prepared by alcohol fractionation before being used for the serological tests. The results demonstrate that such a precipitating system shows characteristics commonly found in antibody-antigen reactions, such as specificity, position of the equivalence point relative to the maximum of precipitation, and solubility of the antibody-antigen complex in excess antigen; this

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All technical papers are published in this section. Manuscripts should be typed double-spaced and be submitted in duplicate. In length, they should be limited to the equivalent of 1200 words; this includes the space occupied by illustrative or tabular material, references and notes, and the author(s)' name(s) and affiliation(s). Illustrative material should be limited to one table *or* one figure. All explanatory notes, including acknowledgments and authorization for publication, and literature references are to be numbered consecutively, keyed into the text proper, and placed at the end of the article under the heading "References and Notes." For fuller details see "Suggestions to Contributors" in *Science* 125, 16 (4 Jan. 1957).

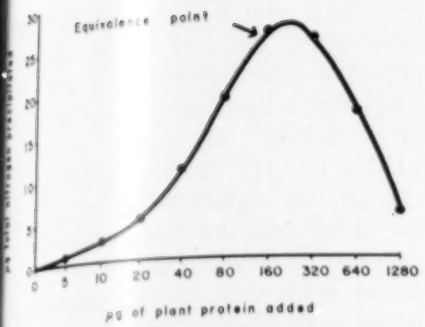

Fig. 1. Amount of nitrogen precipitated in a precipitating system consisting of an alcohol-fractionated-globulin preparation from rabbit antisera and increasingly large quantities of the crude plant protein antigen.

indicates that nonspecific precipitation reactions are absent.

This elimination of the nonspecific precipitation reaction by leaf proteins should make it possible to investigate more adequately by immunochemical methods the proteins from higher plants; such studies, as they relate to pathological conditions, are being continued (*6*).

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6. This report is published with the approval of the director of the Wisconsin Agricultural Experiment Station. The study was supported by grants from the Herman Frasch Foundation and the Research Committee of the Graduate School, University of Wisconsin.
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6 March 1958

Renal Lesions Produced by Group A, Type 12, Streptococci

Group A, types 12, 36, and 3, and group C streptococci (*1*) were grown in Todd-Hewitt broth for 24 hours; each resulting broth culture was inoculated through a glass side arm into a diffusion chamber, and the end of the side arm was sealed in a flame (Fig. 1). The chamber is a modification of the one described by Eschenbrenner and Francis (*2*).

The chambers were fabricated from sheet Plexiglas 3 mm thick. The rectangular pieces with rounded corners measured 20 by 28 mm, and the center hole had a diameter of 12 mm. A hole 1 mm in diameter was drilled in one end, and a 10-mm length of hot capillary tubing was inserted. Membranes of dense porosity (*3*) were glued to each side of the center hole with chloroform-Plexiglas cement. This cement was also used to seal the junction between the glass tubing and the chamber.

Uninoculated and streptococci-containing chambers were placed intraperitoneally in 8-month-old CFW female mice.

It was noted that uninoculated chambers and those containing sterile Todd-Hewitt broth remained in mice for 3 months without evoking renal lesions.

Chambers inoculated with type 12 (nephritogenic) streptococci were implanted intraperitoneally in mice and removed after 24, 48, and 72 hours, respectively. Seven days later the kidneys were removed and the contents of the chamber was cultured to check for contamination. No organisms other than streptococci were found. These kidneys showed proximal tubule necrosis and desquamation of the lining epithelium, moderate thickening of basement membranes, and adhesions between the glomerular tuft and the capsule, with debris and red blood cells in the capsular space. Minimal proliferation of cells of the glomerular tufts was noted (Figs. 2 and 3).

Type 12 (non-nephritogenic) streptococci were implanted as described above, and the chambers and kidneys were removed after the same intervals as in the previous experiments. No evidence of renal lesions was present. To further confirm this, mice were allowed to remain alive for 30 days. At the end of the period no abnormalities were noted in the kidneys.

Type 3 and group C streptococci were used in the above manner without producing renal lesions within 30 days. Isolation of the contents of the chamber revealed pure cultures in each case.

Type 36 streptococci were inoculated into chambers, but leak was purposely made to determine the effect of this type on the mice. Death occurred in 3 to 5 days, with generalized bacteremia; organisms identified as streptococci were isolated from the peritoneal cavity, the blood, and the kidneys. The kidneys were characterized by microabscesses and the picture of acute pyelonephritis. It is interesting to note that implantation of properly sealed chambers bearing type 36 streptococci produced no renal lesions within 1 month.

A streptococcal extract (*4*), prepared after the method of Pappenheimer, when placed in chambers according to the previously described procedure, produced no lesions in the kidneys.

In summary, renal lesions were found in mice bearing diffusion chambers containing nephritogenic type 12 streptococci but not in mice bearing diffusion chambers containing non-nephritogenic type 12 streptococci.

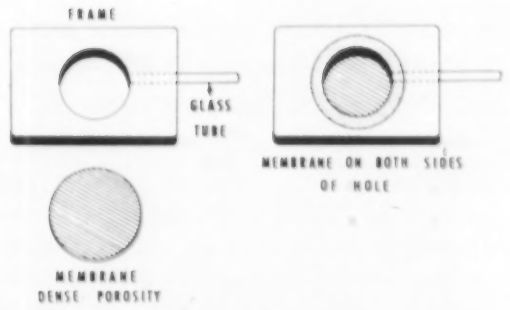

Fig. 1. Modification of chamber described by A. B. Eschenbrenner and R. D. Francis.

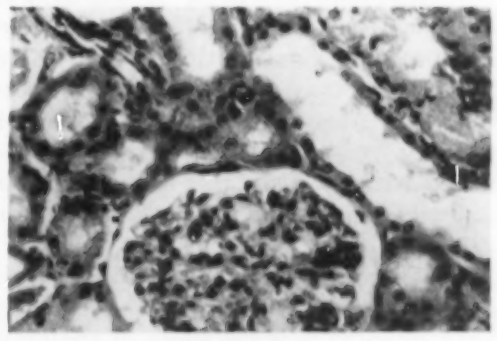

Fig. 2. Photomicrograph of mouse kidney immediately after exposure to type 12 streptococci, in chamber, for 5 days (hematoxylin and eosin stain). (About × 400)

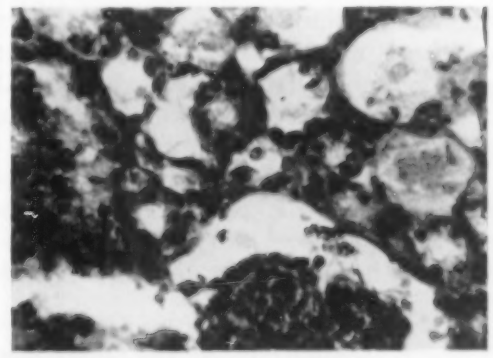

Fig. 3. Photomicrograph of mouse kidney exposed to type 12 streptococci, in chamber, for 48 hours. Kidneys removed 7 days later (hematoxylin and eosin stain). (About × 400)

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References and Notes

1. The following cultures used in this study were furnished by Elaine L. Updyke, Streptococcal Laboratory, Communicable Disease Center, Atlanta, Ga.: Type 12 (nephritogenic) strain DSB-893 isolated from an outbreak of nephritis; type 12 (non-nephritogenic) strain GS-208-4, not known to be associated with nephritis; type 3 strain GS-210-4; type 36 strain SS-269; and type C strain GS-229-4.
2. A. B. Eschenbrenner and R. D. Francis, *Federation Proc.* 15, 514 (1956).
3. The membranes were procured from Schleicher and Schuell Co., Keene, N.H.
4. This extract was prepared from type 12 streptococci, strain DSB-893, by Joseph Schubert, Communicable Disease Center, Atlanta, Ga.

27 January 1958

Red Cell Glucose-6-Phosphate and 6-Phosphogluconic Dehydrogenases and Nucleoside Phosphorylase

Glucose-6-phosphate dehydrogenase, 6-phosphogluconic dehydrogenase, and purine nucleoside phosphorylase are known to be present in mature mammalian erythrocytes (*1–3*). These cells, however, lack many of the enzymes of the tricarboxylic acid cycle (*4*). Accordingly, glucose-6-phosphate and 6-phosphogluconic dehydrogenases, which catalyze reactions providing a mechanism for glucose oxidation to CO_2 with generation of reduced triphosphopyridine nucleotides, may be of particular importance in the metabolic processes of erythrocytes. Purine nucleoside phosphorylase may play a role in the synthesis and degradation of nucleosides in these cells.

Studies of erythrocytes of patients with an increased percentage of reticulocytes have revealed that the levels of cholinesterase, glyoxylase, and carbonic anhydrase, but not purine nucleoside phosphorylase, are elevated (*5*). There are no previous reports of the activity of erythrocyte glucose-6-phosphate or 6-phosphogluconic dehydrogenases in human adult subjects (*6*). In studies in rabbits, Rubinstein *et al.* (*4*) found that glucose-6-phosphate dehydrogenase was about equally active in the reticulocyte and in the mature erythrocyte.

The investigations described in this report (*7*) were designed (i) to measure the levels of these enzymes in erythrocytes of healthy adult human subjects and (ii) to determine whether alterations in the activities of these enzymes occur in erythrocytes of patients with an increased proportion of young red blood cells.

Heparinized venous blood was centrifuged (1500 *g* for 10 min), and the plasma and buffy layer were removed. The erythrocytes were washed twice with isotonic potassium chloride buffered at *p*H 7.4 and diluted to three times the sedimented volume with this solution. This procedure was performed at 0°C. The cells were hemolyzed by freezing and thawing twice. Prior to hemolysis, aliquots of the resuspended cells were removed for counting of erythrocytes (RBC), leukocytes (WBC), and reticulocytes and determination of hemoglobin concentration and hematocrit (*8*). Glucose-6-phosphate dehydrogenase was assayed by a method based on that of Kornberg and Horecker (*9*), 6-phosphogluconic dehydrogenase by a technique similar to that of Horecker and Smyrniotis (*2*), and purine nucleoside phosphorylase by the method of Price *et al.* (*10*) (Table 1). Erythrocytes completely free of leukocytes could not be obtained. Nevertheless, the activities of the enzymes per gram of hemoglobin were not affected by the leukocytes present provided that removal of the buffy layer increased the RBC/WBC ratio to greater than 1000. The presence of ghosts in the hemolysates did not affect the activity of these enzymes.

Individuals with reticulocytosis, compared with normal subjects, had significantly greater activities of glucose-6-phosphate and 6-phosphogluconic dehydrogenases, but not purine nucleoside phosphorylase referred to a unit of hemoglobin, a unit number of erythrocytes, or a volume of packed erythrocytes (Table 1) (*11*). Among the normal individuals, no significant differences were found between the mean values for these enzymes in erythrocytes of males compared with females, Caucasians compared with Negroes, or when age groups were compared by decades from 20 to 70 years. The patients with reticulocytosis included eight with hemolytic anemia of undetermined etiology, six with pernicious anemia, two with sprue, three with sickle-cell anemia, eleven with neoplastic disease, nine with bleeding peptic ulcer, and nine with other conditions associated with anemia and reticulocytosis. All but three individuals with reticulocytosis had elevations in the dehydrogenases beyond the range of the normal values. There was, however, no significant correlation between the degree of reticulocytosis and the activity of either dehydrogenase. In five individuals with macrocytic anemia, a sharp rise occurred in the activities of erythrocyte glucose-6-phosphate and 6-phosphogluconic dehydrogenases, but not in the activity of purine nucleoside phosphorylase, coincident with the reticulocytosis induced by vitamin B_{12} therapy. Although the reticulocyte count returned to normal by the 20th to 25th day, the dehydrogenases remained elevated for 58 to 81 days following institution of therapy.

These findings suggest that elevated levels of glucose-6-phosphate and 6-phosphogluconic dehydrogenases are sensitive indices of erythrocyte populations with a younger than normal mean cell age. In addition, the present data are compatible with the conclusion (*7*) that young, non-reticulated erythrocytes have high levels of these enzymes and that these levels decrease with the aging of the red blood cell in vivo.

Among the 112 healthy individuals (64 Caucasians and 48 Negroes) studied, six (1 Caucasian and 5 Negroes) had levels of erythrocyte glucose-6-phosphate dehydrogenase below three standard deviations of the mean for the remainder of the group. Family studies in these individuals, which have been described in detail elsewhere (*12*), indicate that this erythrocyte enzyme deficiency is genetically determined. The observed decrease in glucose-6-phosphate dehydrogenase in certain healthy subjects is of interest with regard to the demonstration that susceptibility to increased hemolysis following primaquine (*13*) naphthalene or fava bean (*12*) ingestion is associated with an erythrocyte defect in this enzyme.

The finding that glucose-6-phosphate dehydrogenase is deficient in old erythrocytes and in red blood cells particularly

Table 1. Activities of glucose-6-phosphate dehydrogenase, 6-phosphogluconic dehydrogenase, and purine nucleoside phosphorylase in erythrocytes from normal individuals and from subjects with reticulocytosis. The enzyme assays were performed as follows: (i) Glucose-6-phosphate dehydrogenase: 0.1*M* $MgCl_2$, 0.5 ml; 0.25*M* glycylglycine buffer, *p*H 7.6, 0.5 ml; 2.3×10^{-3}*M* triphosphopyridine nucleotide, 0.1 ml; 0.5*M* glucose-6-phosphate, 0.5 ml; hemolysate plus water to 2.5 ml. (ii) 6-Phosphogluconic dehydrogenase: 0.1*M* $MgCl_2$, 0.5 ml; 0.25*M* glycylglycine buffer, *p*H 9.0, 0.5 ml; 2.3×10^{-3}*M* triphosphopyridine nucleotide, 0.1 ml; 0.05*M* 6-phosphogluconate, 0.5 ml; hemolysate plus water to 2.5 ml. Read at 340 mμ. (iii) Purine nucleoside phosphorylase: 0.05*M* phosphate buffer, *p*H 7.4, 2.6 ml; 0.0075*M* inosine, 0.2 ml; xanthine oxidase in excess; hemolysate plus water to 3.0 ml. Read at 293 mμ. Assays were performed in cuvettes with a light path of 1.0 cm. The level of activity of the enzyme is expressed as the change in optical density (ΔO.D.) per minute per gram of hemoglobin, or per 10^6 erythrocytes, or per milliliter of packed erythrocytes, as indicated.

Normal subjects		Subjects with reticulocytosis	
No.	ΔO.D./min (mean ± S.D.)	No.	ΔO.D./min (mean ± S.D.)
	Glucose-6-phosphate dehydrogenase		
112	15.9 ± 2.4/g of hemoglobin	47	29.7 ± 2.4/g of hemoglobin
94	0.54 ± 0.12/10^6 erythrocytes	36	1.09 ± 0.48/10^6 erythrocytes
76	4.95 ± 0.65/ml of erythrocytes	29	8.86 ± 1.24/ml of erythrocytes
	6-Phosphogluconic dehydrogenase		
66	17.3 ± 3.90/g of hemoglobin	31	36.2 ± 8.70/g of hemoglobin
54	0.66 ± 0.15/10^6 erythrocytes	26	1.29 ± 0.51/10^6 erythrocytes
46	6.41 ± 1.04/ml of erythrocytes	21	11.41 ± 8.10/ml of erythrocytes
	Purine nucleoside phosphorylase		
52	145 ± 21.0/g of hemoglobin	28	165 ± 38/g of hemoglobin
44	5.51 ± 0.87/10^6 erythrocytes	19	6.48 ± 2.01/10^6 erythrocytes
37	43.6 ± 3.10/ml erythrocytes	16	53.9 ± 7.9/ml erythrocytes

susceptible to hemolysis by certain drugs suggests that the activity of this enzyme may be an important factor in the maintenance of the integrity of these cells.

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7. A portion of the present data was presented before the Society for Clinical Investigation in Atlantic City, N.J., in May 1957, and has appeared in abstract form [P. A. Marks, *J. Clin. Invest.* 36, 913 (1957)]; P. A. Marks *et al.*, *Proc. Natl. Acad. Sci. U.S.*, in press. This study was supported in part by grant C-2332 from the National Cancer Institute.
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12 December 1957

Three Chromosome Numbers in Whites and Japanese

Since Painter (*1*) reported in 1923 that the chromosome number in man is 48, this has been confirmed by a number of authors (*2*). This number (48) has had general acceptance except by the Japanese school, which has followed de Winiwarter and Oguma's report (*3*) that the Y-chromosome does not exist in man and that the total number of chromosomes in the male is 47. Recently a different number, 46, has been found by Tjio and Levan (*4*) in cultured tissues of four Swedish embryos. The same number of chromosomes (46) was discovered by Ford and Hamerton (*5*) in the testes of three Whites in England and by me and my coworkers (*6*) in the testes of Japanese. There is no doubt that this number (46) exists in man, but this is not the only possible number in the species; we (*6*) found, besides individuals with 46 chromosomes, some with 47 and others with 48 among Japanese.

A new group of Japanese was studied recently to extend the earlier investigation (*7*). A group of Whites was also studied to determine whether the same chromosomal variation exists in this ethnic group (*8*). The material examined consisted of tissue from testes of 15 Japanese (epididymitis patients), secured by biopsy, and testes from eight American Whites (prostate cancer patients), secured by total orchidectomy. Immediately upon removal, the specimens were pretreated with a mixture of equal volumes of 1-percent chromic acid and 3-percent potassium bichromate solutions for about 1½ hours. Specimens were then fixed with a mixture of equal volumes of 4.5-percent chromic acid and 1.5-percent potassium bichromate solutions for 17 to 20 hours. After being washed thoroughly in running water, they were stained by Feulgen's method and squashed. The pretreatment in this procedure facilitates the dispersion of the chromosomes in squashed metaphase cells. Some of the White testes showed slight fibrosis, but otherwise no testes showed indications of pathologic changes. At least 15 first meiotic metaphases and three or four spermatogonial metaphases, in which the chromosomes were dispersed well in the cell and could be observed clearly, were selected in each specimen. The number of chromosomes as well as the structure of individual chromosomes was carefully studied.

In nine of the 15 Japanese testes the spermatogonial metaphases showed 46 chromosomes, and the first meiotic metaphases showed consistently the heteromorphic X-Y pair and 22 paired autosomes. While the autosomal pairs always formed tetrads, the sex chromosomes were separate from each other in 40 percent of the first metaphases. The pairing irregularity of the X-Y pair was found to occur also in the other six Japanese and the eight White testes.

In one of the 15 Japanese testes, the number of chromosomes was found to be consistently 47 in the spermatogonia and first meiotic metaphases. In the latter metaphases a small univalent was always present in addition to the X-Y and the 22 autosomal bivalents. There was no indication that the univalent chromosome was produced by fragmentation of one of the sex chromosomes or of one of the autosomes. It is an intact chromosome with its own centromere. The X-Y and the 22 autosomal bivalents in this testis were compared with those in the nine testes with 46 chromosomes, and all chromosomes were found to match well in size and shape. This indicated that the univalent chromosome in the testis with 47 chromosomes is an extra element present in addition to the 23 pairs that constitute the regular complement in 46- and 47-chromosome individuals.

In the remaining five of the 15 Japanese testes, 48 chromosomes were found in spermatogonial metaphases, and the heteromorphic X-Y pair and the 23 homomorphic bivalents were found in all first meiotic metaphases. The matching of these bivalents with those of the 47-chromosome individuals indicated that one of the 23 bivalents corresponds to the univalent chromosome of the latter individual. Evidently the extra chromosome present singly in the 47-chromosome individual is duplicated in individuals of the 48-chromosome type. Thus, the Japanese comprise individuals of three different chromosomal constitutions, and the differentiating factor is the chromosome which occurs singly in some individuals but as a duplicate in others. This chromosome appears to be a supernumerary chromosome.

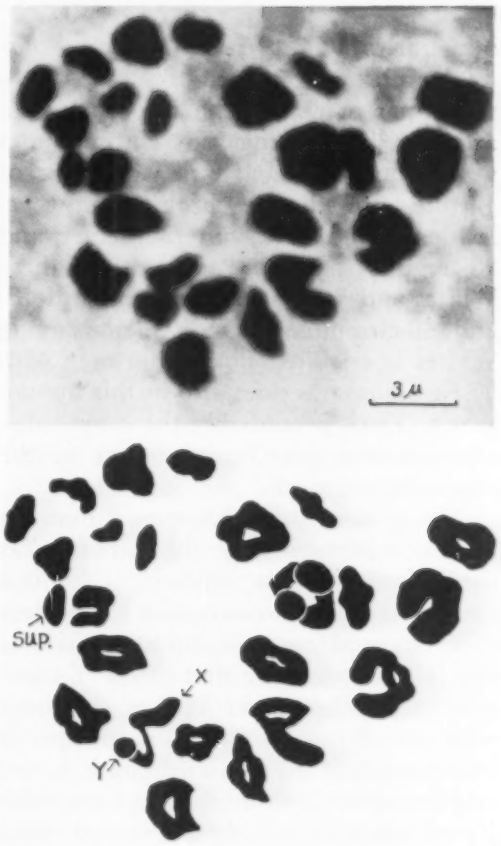

Fig. 1. A first meiotic metaphase of a White male with 48 chromosomes. Note the X-Y pair, the 22 autosomal pairs, and a bivalent supernumerary chromosome (*sup.*).

In seven of the eight White testes, 46 chromosomes were present consistently in all spermatogonial and primary spermatocyte metaphases. In size and shape the individual chromosomes were essentially the same as the 46 chromosomes in Japanese of the same type. In the remaining one of the White testes, 48 chromosomes were found in spermatogonial metaphases and the X-Y and the 23 bivalents were found in all first meiotic metaphases (Fig. 1). In Fig. 1 (top), the supernumerary bivalent appears to be connected to the adjacent bivalents. However, extensive observations of both Japanese and White materials have never indicated any tendency for the super-

numeraries to associate with any particular chromosome. In this case, then, the apparent connection is probably an artifact. These meiotic chromosomes were found to match well with those of Japanese of the 48-chromosome type. Evidently the same supernumerary and 23 regular pairs are present in these Whites and Japanese. The finding of 46- and 48-chromosome individuals among Whites leaves little doubt that men with 47 chromosomes exist also in this human group. Thus, presumably the same three chromosomal constitutions exist in the two ethnic groups.

The present study has shown that the human supernumerary chromosome has the following characteristics: (i) It is a metacentric chromosome with the centromere located near its middle; (ii) its size is approximately that of the Y-chromosome; (iii) it never pairs or associates with any other chromosome except its own homolog; (iv) at metaphase I, two supernumerary chromosomes are conjoined more frequently at one arm than at two arms. In either case the attachment of the arms is always completely terminal, and the attachment region is sometimes strikingly attenuated. A similar manner of pairing is observed in the X-Y pair but not in other autosomes.

Since 1956, 15 individuals have been reported to have 46 chromosomes, including the recent one studied by Bender (*9*) and the seven described here. The White individual with 48 chromosomes in our sample is the only one with this number established since 1956. The ratio of the frequency of the 46- and 48-chromosome types in the present Japanese sample is 9:5 (a previously reported ratio, 4:16, was not based on a random sample). The numbers of Whites and Japanese studied so far are too small to provide the basis of reliable estimates of the frequencies of the three karyotypes in the two ethnic groups.

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17 January 1958

Continuous Measurement of the Depth of Sleep

Electroencephalographic records characterizing levels of natural sleep in human beings (*1*, *2*) appear to be marked by a diminution of the number of brain-wave peaks during the passage from wakefulness to deepest sleep. The present report deals with the validation of this observation.

Bickford (*3*) has used the integrated energy of the electroencephalographic output to control the level of anesthesia, the energy decreasing with the depth of narcosis. Forbes *et al.* (*4*) have measured the depth of barbiturate narcosis, in animals already lightly anesthetized, by counting manually the number of brain waves that exceed one-third the maximum amplitude in successive 40-second intervals. This brain-wave count appeared to measure the absolute depth of narcosis more reliably than did the Bickford energy record.

The measurement described in the present report differs from that of Forbes *et al.* in employing nonnarcotized human beings; in producing a continuous 12 in./hr record of the electroencephalographic frequency during sleep; and in obviating the need for knowledge of the eventual maximum amplitude of the brain-wave potentials. The use of the energy-output method would probably fail during natural sleep because such electroencephalographic records do not reveal a proportionality between energy output and depth of sleep.

The brain-wave potentials during sleep are obtained from two needle electrodes, placed ipsilaterally over the occipital and frontal areas of the scalp, with a third silver disc electrode attached to the ear lobe on the same side and grounded. This electrode placement gives maximum alpha and delta waves.

The potentials were amplified by a Type 122 Tektronix low-level preamplifier, the low impedance output being fed into an electrocardiograph, whose "voltage gain" was set so that the maximum amplitude of the alpha rhythm in the waking state was 7 volts. This amplified signal then passed into a Schmidt trigger circuit set to pass only positive-going pulses greater than 2 volts. The square-wave output of the Schmidt circuit actuated an electronic counter and rate transducer (*5*) to produce, finally, a record of frequency versus time on a Leeds and Northrup Speedomax recorder. The frequency scale was set to cover the range from 3 to 16 cycles per second on the 10 in. recorder paper; the relationship between millivolts and the reciprocal of frequency is linear. The electronic counter keyed a relay every 32 counts; the transducer thus produced a rate based on the mean of successive trains of 32 positive-going waves of greater than 2 volts amplitude.

If the amplified output of the alpha rhythm is set lower than 7 volts or if the trigger threshold is set higher than 2 volts, the so-called transition or "B" stage of sleep (*2*), a relatively fast rhythm of low amplitude, will appear to be of low frequency and will consequently be interpreted as a deeper stage of sleep. There is no reason to believe that marked differences will occur if this ratio is varied between 3:1 and 4:1.

The use of the electroencephalographic frequency as a continuous measure of the depth of sleep was validated as follows: At intervals of 8 to 10 minutes throughout the night a 30-second written record of the electroencephalographic potentials was obtained on the strip chart of the electrocardiograph; the time at which this record was taken was signal-marked on the continuous electroencephalographic frequency recording. This record was then cut in half, and each 15-second strip was randomly numbered; at the end of the night, 100 such strips had been accumulated. These strips were then shuffled so that their subsequent order would differ from the order in which they had been obtained. Three judges then independently evaluated each record on a scale of 4, zero being the waking state and 1, 2, and 3 being light, moderate, and deep sleep, respectively. Prior to classifying the records, each judge was shown sample sleep records from the electroencephalographic literature. After 24 hours or more had elapsed the judges again evaluated the same records after they had been shuffled into a different order. Thus, each 30-second record received six scores on each of two occasions. The phi correlation coefficient was computed for the means of the first and second judgments and in three different subjects was found to be 0.88, 0.88 and 1.00, respectively; in the first two cases this particular correlation coefficient probably indicated too low a degree of reliability of the judgments. The phi correlation coefficient was then computed as a validity coefficient for the correspondence between the mean of the 12 judgments of each record and the corresponding electroencephalographic frequency. For the two males and one female of this study, this coefficient was 0.76, 0.75, and 0.91, respectively. Although highly significant, this coefficient probably indicates too low a degree of reliability.

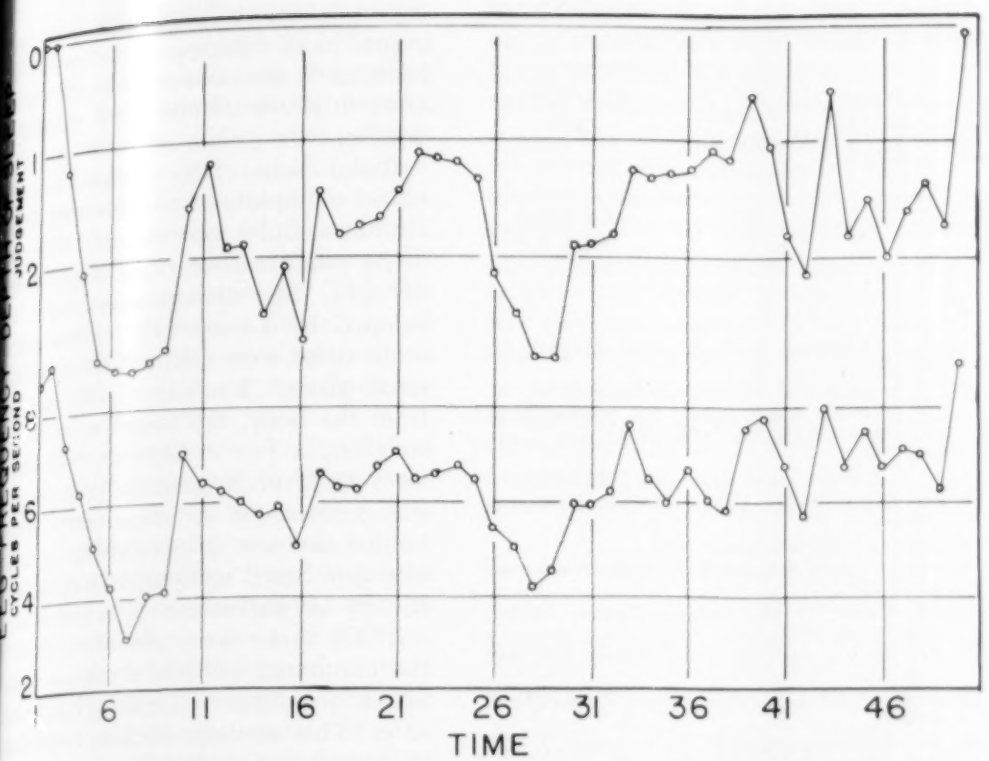

Fig. 1. (Lower curve) Electroencephalographic frequency during a night's sleep. (Upper curve) Judgments of depth of sleep made from the electroencephalogram, at corresponding times.

Figure 1 represents the depth of sleep of one subject during the night, as determined both by measurement of the electroencephalographic frequency and simultaneous scoring of the electroencephalogram. For this subject the phi correlation coefficients were 0.88 and 0.76.

If one assumes that there is basis for the belief that the electroencephalographic frequency represents the continuum of sleep depth, the use of a limited number of categories (observers' judgments) for correlation does not offer the best evidence for this assumption. It also appears somewhat circular to validate the electroencephalographic frequency with a measure from the same class. In the past many observers have noted, though not with the greatest rigor, that the electroencephalogram is correlated with the strength of stimulus necessary for arousal. The electroencephalographic frequency as measured here offers the possibility of making such a test by correlating frequency with strength of arousal stimulus, because such stimuli may be divided objectively into more categories than can observers' judgments of the electroencephalogram. Since the meaning of various levels of sleep depth is commonly believed to be associated with the reactivity of the nervous system, a correlation between the two continua would infuse both measures with additional meaning. The meaning would depend on the nature of the relationship and the definition of "depth of sleep."

In three other subjects preliminary observations were made as to the correspondence between minimum auditory awakening stimulus and electroencephalographic frequency. Although these observations are too few in number to have any substantive value, not one of them is at variance with the basic postulate: the lower the electroencephalographic frequency the louder the sound stimulus necessary for arousal.

Lindsley's (*6*) ingenious use of operant behavior may be related to the common concept of sleep depth; it suffers from the disadvantage of imposing continuously upon the sleeper either a sound stimulus or the necessity of his pressing a switch. The importance of using the electroencephalographic record to evaluate the operant response has been recognized by Lindsley.

The present method of continuous registration of the electroencephalographic frequency has been shown to represent with high reliability the time course of the depth of sleep through the night; additional evidence for this correlation should be sought by the use of another independent measure. Since the measurement may be carried out in the nonnarcotized individual, without the application of any external stimuli and with no discomfort to the sleeper, it offers many advantages to investigators in this field.

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29 January 1958

Effect of Chloramphenicol on Glucose Oxidation in Escherichia coli

When bacteriostatic concentrations of chloramphenicol are added to growing cultures of susceptible bacteria, protein synthesis and therefore reproduction are inhibited, while other cell processes continue at the same rate or at somewhat slower rates (*1*). Some specific reactions, however, such as indole synthesis in *Escherichia coli* (*2*) and oxidation of organic acids in *Pseudomonas fluorescens* (*3*), are also inhibited. One might expect inhibition by chloramphenicol to effect a shift in any metabolic reactions associated with protein synthesis and growth. Cohen (*4*) suggested that *E. coli* oxidizes glucose chiefly by the hexosemonophosphate shunt during growth and by the Embden-Meyerhof pathway during the "resting" state. The findings presented in this report show that inhibition by chloramphenicol does indeed alter the activity of a C-1 preferential pathway of glucose oxidation by growing cells of *E. coli*.

In all experiments *E. coli* was grown at 37°C in synthetic medium consisting of 5.4 g of KH_2PO_4, 1.2 g of $(NH_4)_2HPO_4$, 0.2 g of $MgSO_4 \cdot 7H_2O$, 1.5 g of glucose, and glass-distilled water to make up 1 lit, at *p*H 7.1. The inoculum was grown in synthetic medium for two transfers before use and was added to growth flasks to give 3×10^8 cells per milliliter. Manometric and $C^{14}O_2$ determinations were made in double-sidearm Dixon-Keilin Warburg flasks. Total cell counts and substrate uptake during growth were measured in conventional double-sidearm Warburg flasks. Determinations were made for the 30-minute period before as well as after addition of chloramphenicol (final concentration, 60 μg/ml). Flasks for each manometric and isotopic determination were run in duplicate. Total cell counts were made with a Petroff-Hausser chamber and a bright high-contrast oil-immersion objective of a phase contrast microscope. Residual glucose was determined by the anthrone method (*5*, *6*), ammonia nitrogen by the phenol hypochlorite test (*6*). The $C^{14}O_2$ collected was precipitated with a saturated $Ba(OH)_2$–10-percent $BaCl_2$ solution, plated on a porcelain disk, and counted with a windowless gas flow counter (*7*). The counts were corrected for self-absorption (*8*) and background. Aliquots

Table 1. Effect of chloramphenicol on the metabolism of growing *E. coli* during the first 30 minutes of treatment. Average deviation for glucose, O_2, or $CO_2 = 4$ percent.

Chloramphenicol (μg/ml)	Increase in total count (10^8 cells/ml)	NH_4-N uptake (μg/ml)	Glucose uptake (μmole)	Oxygen uptake (μmole)	Carbon-dioxide evolution (μmole)	C-1/C-6 ratio
0	4.7 to 6.0	13	2.3	5.4	6.0	12.2 ± 0.2
60	4.7 to 5.0	2	1.5	3.2	3.7	5.1 ± 0.6

of glucose-C^{14} substrates were combusted to $C^{14}O_2$ by the persulfate method (*9*). The activity of a C-1 preferential pathway was measured (*10*) by the ratio: (percentage of radiochemical yield of $C^{14}O_2$ from glucose-1-C^{14})/(percentage of radiochemical yield of $C^{14}O_2$ from glucose-6-C^{14}).

In preliminary experiments, data showed that chloramphenicol-treated cells took up less glucose than cells in control cultures and oxidized less of the glucose by a C-1 preferential pathway; the values of the ratio C-1/C-6 were 10 to 16 for the controls and about 5 for the treated cultures. In Table 1 are presented results of an experiment in which nitrogen assimilation was almost completely inhibited and glucose assimilation and oxidation continued during treatment of cultures with the antibiotic. The inhibited cultures used less glucose (65 percent as much as the controls) but assimilated the same proportion, about 60 percent. However, there appeared to be a shift in the proportion of glucose oxidized by way of a C-1 preferential pathway, as evidenced by the C-1/C-6 ratios. A large percentage of the glucose oxidized still was degraded by the C-1 selective mechanism. In the first 30 minutes of treatment, synthesis of deoxyribonucleic acid was slightly affected, while the synthesis of ribonucleic acid was 55 percent of that of the controls (77 percent on a "per cell" basis).

Further experiments are necessary to establish the sequence of events here. However, previous experiments (*11*) have shown that when limiting amounts of nitrogen halt the growth of cultures of *E. coli*, a similar shift of a C-1 preferential pathway in glucose metabolism occurs. In that case, too, assimilation of glucose continues, and so does synthesis of nucleic acids for a time (*12*). Possibly the interference with the assimilation of nitrogen by chloramphenicol indirectly affects the activity of the C-1 preferential pathway of glucose oxidation.

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16 September 1957

Quantitative Histochemistry of the Nephron

The broad purposes of our investigations are to determine quantitatively the activity of enzymes and the concentrations of various substances in the anatomic units of the human kidney in health and disease. This report describes the technique used to identify and dissect out various microscopic portions of the nephron. The accuracy of the method was checked by determining the distribution and concentration of alkaline phosphatase in different parts of the nephron, because histologic staining techniques have demonstrated clear-cut differences in the concentration of this enzyme in proximal and distal convoluted tubules.

Renal tissue from animals was obtained as rapidly as possible after death. Human samples were obtained at autopsy or by percutaneous renal biopsy during life (*1*). The ultramicrotechniques developed by Lowry (*2*) for analysis of brain tissue were adapted for analysis of renal tissue. Immediately on removal from the body, the fragment of kidney, weighing a few milligrams, was placed on a layer of tragacanth jelly covering the surface of a microtome sample holder and was frozen rapidly in liquid nitrogen. Serial sections, 16 μ thick, were cut by a microtome in a cryostat at −20°C; these were placed seriatim in the numbered wells of a section holder which was inserted into a lyophilizing tube. The sections in the lyophilizing tube were frozen-dried in the cryostat at −20°C, and the evacuated tube was stored, until needed, in a freezer at −35°C.

Alternate sections of frozen-dried tissue were stained by means of the periodic-acid-Schiff technique, counterstained with hematoxylin, and studied under a microscope equipped with a Galileo projection viewer. The whole section was mapped out, blood vessels and glomeruli serving as landmarks. The convoluted tubules were recognized by their relative proximity to the glomerulus. The proximal convoluted tubules were identified by the presence of a brush border, by the small number of nuclei (4 to 6) in cross section, and by their relatively tall cells. The distal convoluted tubules were distinguished from them by their intimate relationship to the macula densa, by the absence of a brush border, by the greater number of nuclei (8 to 10)

Table 1. Distribution of alkaline phosphatase in the kidney, expressed in moles of *p*-nitrophenylphosphate split per kilogram (dry weight) per hour, with standard deviation and in parentheses, the number of fragments analyzed.

Structure	Tissue sample					
	Dog		Rat		Man	
	No. 1	No. 2	No. 1	No. 2	No. 1*	No. 2†
Glomerulus	1.9 ± 0.3 (8)‡	1.8 ± 0.6 (5)§	6.6 ± 2.9 (5)	4.3 ± 2.7 (6)	0.4 ± 0.2 (4)	2.1 ± 1.7 (5)
Proximal convoluted tubules	10.4 ± 4.3 (13)	10.3 ± 3.5 (6)	16.9 ± 9.5 (7)	17.4 ± 6.5 (11)	6.8 ± 1.8 (6)	6.7 ± 1.1 (9)
Distal convoluted tubules	1.6 ± 1.5 (5)	0.3 ± 0.2 (5)	0.8 ± 0.4 (6)		3.2 ± 1.6 (5)	3.2 ± 0.9 (8)
Medullary ray	0.4 ± 0.4 (8)	0.3 ± 0.4 (5)	0.6 ± 0.5 (4)	0.4 ± 0.8 (5)		
Medullary tubules			1.0 ± 1.0 (6)			
Papillary tubules:				0.5 ± 0.6 (6)		1.2 ± 1.0 (11)
Base		0.3 ± 0.0 (2)	0.4 ± 0.2 (4)			
Apex			1.5 ± 0.7 (3)	0.9 ± 0.9 (6)		2.2 ± 1.3 (3)
Vessels	0.6 ± 0.1 (4)‖	0.1 ± 0.1 (4)‖		15. 5 ± 4.4 (6)		

* Biopsy from an adult male. † Autopsy specimen obtained 6 hr after death, from an adult female. ‡ Bowman capsule: ± 0.2 (1). § Glomerular vessels: 0.3 ± 0.5 (2). ‖ Vessel wall plus fibrous area: 0.6 ± 0.1 (4); vessel wall only: 0.1 ± 0.1 (14).

cross section, and by their relatively w cells. Henle's loop and the collecting ubules were recognized in the medullary y and in the medulla; in the papilla, llecting tubules only were found. hen the various parts of the kidney d been identified in the stained section, ey were dissected out of the adjacent stained section with microscalpels nder a dissection microscope (magnifiation ×40). Separate dissection of Henle's loops and collecting tubules was rely possible because of their intimate natomical relationship.

The dissected specimens were weighed n a quartz fiber fish-pole balance (sentivity 0.4 mμg; useful range 10 to 100 μg) (*2*). A manipulator made from a icroscope mechanical stage was used o load and unload the balance pan. A orizontal microscope fitted with a verical fine adjustment was used to read he displacement of the pan. Under the issection microscope, each of the eighed specimens was transferred to he bottom of micro test tubes (inside iameter, 3 mm) by a fine glass needle. oth dissection and weighing were done n a room maintained at low humidity nd relatively constant temperature.

The specimens were assayed to deterine alkaline phosphatase activity by dding 3 μl of substrate reagent [0.5*M* -amino-2-methylpropanol-1 (*p*H 10.0), mmole/lit of *p*-nitrophenylphosphate, mmole/lit of 1*M* $MgCl_2$, and 0.05-percent bovine serum albumin (*3*)]. After 1 our of incubation at 37°C, the reaction as stopped and color was developed by dding 50 μl of 0.1*N* NaOH. The optical ensity of the solution was read in owry-Bessey microcuvettes (*4*) at 410 mμ in the Beckman DU spectrophotomter, and the results were expressed in oles of substrate split per kilogram (dry eight) per hour.

Typical data for individual kidneys of og, rat, and man are shown in Table 1. he results within each species were conistent, although somewhat different disributions were found in the tubules of ach species. The experimental reproucibility for renal homogenates, expressed as standard deviation, was found o be ±0.8 moles split per kilogram dry weight) per hour. The standard eviation for individually assayed proximal convoluted tubules in each kidney as much greater than this, indicating hat the alkaline phosphatase activity of hese tubules varied considerably between individual nephrons.

The results indicate accurate identification and dissection of the various parts of the nephron, particularly the proximal and distal convoluted tubules. For example, the values obtained for dog kidney are comparable to those of McCann (*5*), who used vital staining with trypan blue to differentiate proximal and distal convoluted tubules in the dog. Unfortunately this and other dyes which localize in the proximal tubules are too toxic for use in man, while fluorescence microscopy, phase microscopy, and polarization microscopy did not allow a distinction between the two types of convoluted tubules at the low magnifications which must be used with the dissection microscope. The technique presented here (*6*) has the advantages of being generally applicable, of allowing accurate identification and dissection of the desired structures, and of providing a permanent record in the form of the stained sections and maps.

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6. We are greatly indebted to Dr. Oliver H. Lowry for his help, especially for arranging for one of us (S.L.B.) to work in his department and learn some of the techniques employed in this investigation. The technical assistance of Miss Alta D. Tsoodle is gratefully acknowledged. This study was supported by contract No. DA-49-007-MD-637, Research and Development Division, Surgeon General's Office, U.S. Department of the Army.

13 January 1958

Preliminary Note on Kimzeyite, a New Zirconium Garnet

In 1953, during geological study of the Magnet Cove, Arkansas, carbonatite, R. L. Erickson and L. V. Blade, geologists of the U.S. Geological Survey, noted well-crystallized dark brown garnets about 5 mm in diameter in calcite rock in the Kimzey Calcite Quarry. With the garnets was a variety of other minerals, including monticellite, magnetite, perovskite, and apatite. The garnets themselves are shot through with those minerals, and in addition, with sharply euhedral, almost microscopic, crystals of anhydrite.

In 1953, A. T. Myers, spectrographer, U.S. Geological Survey, reported more than 10 percent zirconia, which led to further examination of the garnet. Myers' work was confirmed in 1954 by H. J. Rose (U.S. Geological Survey) who, on another sample, found zirconium and titanium in the X percent range, XO percent Fe and Ca, and O.X percent Mg and Al.

Table 1. Composition of kimzeyite. Looked for but not found: Ag, Au, Hg, Ru, Rh, Pd, Ce, Ir, Ge, Pb, As, Sb, Pt, Mo, W, Re, Bi, Zn, Cd, Te, In, Co, Ni, Ga, Cr, V, Y, La, Hf, Th, Ta, Be, Li, Na, K, B. Not tested for: P_2O_5, H_2O, F, S, and CO_2.

Element	Amt. %	Computed as oxide	
		Oxide	(%)
Si	10	SiO_2	21.4
Al	6	Al_2O_3	11.4
Ca	12	CaO	16.8
Fe	11.5	Fe_2O_3	16.45
Ti	3.5	TiO_2	5.8
Zr	15	ZrO_2	20.25
Nb	0.5	Nb_2O_5	0.72
Mg	0.3	MgO	0.5
Mn	0.1	MnO	0.13
Sn	0.07	SnO_2	0.09
Sc	0.06	Sc_2O_3	0.09
Cu, Ba, Sr	trace		
Total			94.

Finally, 35 mg of microscopically clean garnet was analyzed by Harry Bastron, spectrographer, U.S. Geological Survey. His analysis is shown in Table 1.

An x-ray diffraction pattern by F. A. Hildebrand (U.S. Geological Survey) showed "garnet group mineral; no perovskite detectable." The cell edge measured by J. M. Axelrod (U.S. Geological Survey) is $a_0 = 12.46$ A.

The garnet in thin section is isotropic, light brown, and has an index of refraction near 1.95. This is substantially higher than the index of refraction (1.895) of andradite, the pure calcium iron garnet, but is within the range of the indices of refraction of the calcium iron titanium garnets, schorlomite-ivaarite, which range up to 2.01.

Further work is in progress on this mineral, which is here named "kimzeyite." The Kimzey family has been actively associated with mineralogical developments in Magnet Cove for almost a century. Museums all over the world owe some of their best specimens of the remarkable Magnet Cove minerals to the intelligent zeal of the Kimzey family, notably William J. Kimzey, his son Joe Kimzey, former state geologist of Arkansas, and Lawton D. and John Kimzey (*1*).

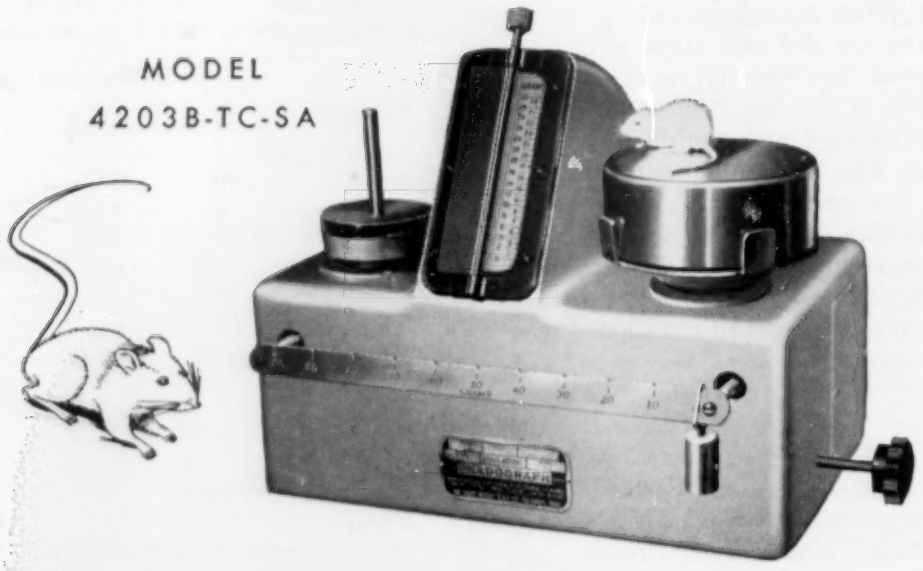
CHARLES MILTON
LAWRENCE V. BLADE
U.S. Geological Survey, Washington, D.C.

Note

1. Publication authorized by the director, U.S. Geological Survey.

11 December 1957

Positive stop readings in 1.13 seconds



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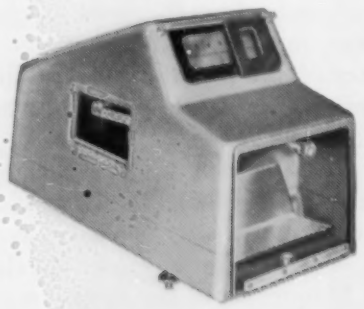

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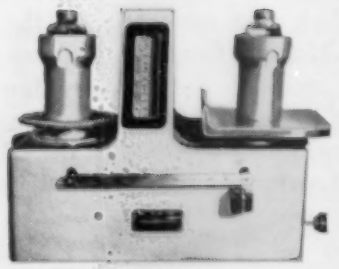

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Meetings

Science and Mathematics Teacher Education

The Mideast Regional State Colle Conference on Science and Mathema Teacher Education was held in Washi ton, D.C., on 7 and 8 March 1958, un the sponsorship of the Science Teachi Improvement Program of the Americ Association for the Advancement of S ence. (The conference was made po ble through grants from the Carne Corporation of New York and the Ge eral Electric Educational and Charita Fund.) The original stimulus for t conference was provided by the succes ful completion of a similar conferenc also sponsored by the Science Teachi Improvement Program, held in the Mi west last year. The purpose of these co ferences is to bring together represent tives of the mathematics and scien faculties of the state colleges in orde that they may have the opportunity t exchange views with others concerni current developments in science and i better teaching of science. Since a lar percentage of the future teachers in o public schools will be produced in the state colleges, and since experimenta programs in education are being co ducted in many of them, it is also de sirable that representatives from the institutions meet with representatives o the organizations interested in improvi the teaching of science in the hig schools.

Participants in the Mideast Confer ence included representatives of the sc ence and mathematics departments o state colleges of the District of Colum bia, Maryland, New Jersey, New York Pennsylvania, Virginia, and West Vir ginia; high-school supervisors of scienc and mathematics from the Washingto metropolitan area; and other leaders i science and mathematics education. I all, approximately one hundred person participated in the conference.

The conference was opened with a challenge to face new times and new tasks with an open mind and a deter mination to develop the best possible educational system for American democ racy. Topics developed in subsequent addresses included: science and mathe matics in the Soviet ten-year school; a review of current experimental program in the United States in science and mathematics teaching, particularly at the secondary level; a plea for coordination of the science programs at the elemen tary, secondary, and collegiate levels; suggestions for improvements in cur ricula for the training of teachers of science and mathematics; and recom mendations for revision of secondary school curricula in science and mathe

...ics to meet the rapid advances currently observed in these fields.

No resolutions were adopted by the conference, but recommendations of the discussion groups in both mathematics and science included the following:

(1) Programs of preparation for teaching of mathematics and science should be designed to provide strong concentration (one-half of the total number of college hours) in the general academic area to be taught, with sufficient concentration in a specific subject to prepare the teacher for graduate work in that field.

(2) All teachers for elementary schools should have training in biological sciences, physical sciences, and mathematics equal to at least a one-year college course in each.

(3) It is strongly recommended that further experimental studies of the curriculum content of both high-school and college courses in mathematics and science be made in an effort to improve training in these fields.

(4) An organized program providing adequate counseling service in mathematics and science for secondary teachers should be established in all school systems. This program might be modeled after the STIP program.

(5) Teaching loads should be reduced to a reasonable level, with adequate allowance provided for time spent on extracurricular activities. In figuring work loads, total clock hours should be used as the criterion, with laboratory hours carrying equal credit with class hours.

(6) Colleges should establish special-content courses in science and mathematics for in-service training of teachers. These courses should provide the breadth of knowledge required in teacher training, and should carry graduate credit.

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American Association for the Advancement of Science, Washington, D.C.

Engineering Education

The American Society for Engineering Education will hold its annual national meeting from 16 to 20 June on the Berkeley campus of the University of California. During that week more than 1000 engineers and educators will gather to present and discuss recent progress toward increasing the effectiveness of engineering and applied science instruction in the nation's schools and colleges.

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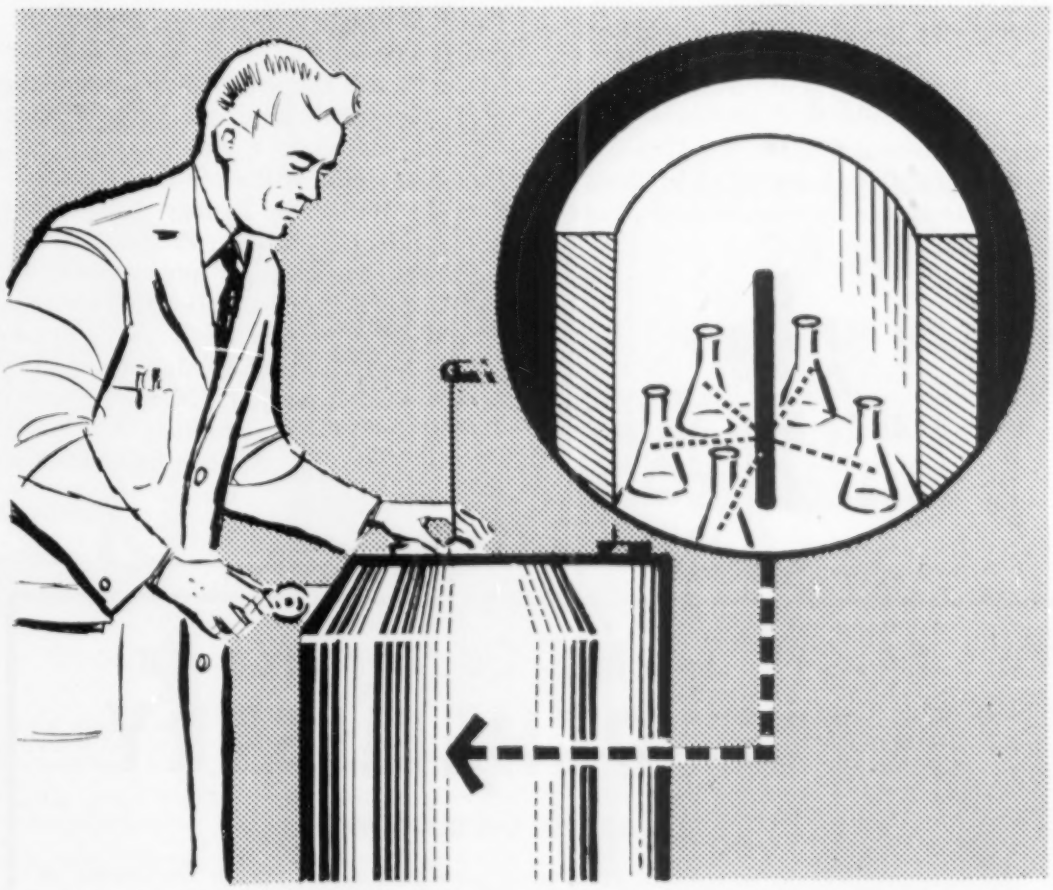

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Australasian Conference on Radiation Biology

The second Australasian Conference on Radiation Biology will be held at the Cancer Institute, Melbourne, Australia, 15–18 December. Guest speakers at the conference will be L. H. Gray, director of the British Empire Cancer Campaign, Radiobiological Research Unit, Mount Vernon Hospital, London, and J. F. Loutit, director of the Medical Research Council Radiation Biology Unit, Harwell, Didcot, Berkshire, England.

Papers on relevant subjects are invited. Titles and a 250-word abstract should be submitted *by 31 July*. Registration forms and other information may be obtained from J. H. Martin, Physics Department, Cancer Institute Board, 483 Lt. Lonsdale St., Melbourne, Victoria, Australia.

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Extended abstracts, in English, of 121 papers presented at the Fourth Conference on Electrochemistry held in the USSR, October, 1956. This Conference brought together most of the Soviet Union's most eminent electrochemists, to review progress since the 3rd (1950) Conference. Abstracts will not be sold singly, but a Table of Contents will be sent free on request. The scope of the meeting may be inferred from the following list of topics covered, and the number of papers for each:

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Waksman Birthday Meeting

A Symposium on Microbiology—Yesterday and Today was held on 5 June at the Institute of Microbiology, Rutgers University, in honor of Selman A. Waksman on the occasion of his 70th birthday. Attendance will be limited to 200. For information write to Edward R. Isaacs, Rutgers Institute of Microbiology, New Brunswick, N.J.

Forthcoming Events

July

2–5. Ferro and Antiferromagnetism, IUPAP Colloquium (by invitation), Grenoble, France. (L. Neel, Dept. of Experimental Physics, Univ. of Grenoble.)

2–5. Rarefied Gas Dynamics Symp., Nice, France. (F. M. Devienne, Laboratoire Mediterraneen de Recherches Thermodynamiques, 2 avenue Villebois Mareuil, Nice.)

4–6. Astronomical League, Ithaca, N.Y. (Miss W. A. Cherup, 4 Klopfer St., Millvale, Pittsburgh 9, Pa.)

4–6. Speleology, intern. colloquium, Brussels, Belgium. (S. Paumen, 183, avenue Nouvelle, Brussels.)

6–12. Cancer Cong., 7th intern., London, England. (H. F. Dorn, National Inst. of Health, Bethesda 14, Md.)

6–12. Research and Development Engineering Seminar, 2nd annual, University Park, Pa. (Extension Conference Center, Pennsylvania State Univ., University Park.)

7–9. Exchange of Knowledge in a Divided World, Chicago, Ill. (H. W. Winger, Graduate Library School, Univ. of Chicago, Chicago 37).

7–11. Technical and Industrial Communications Inst., Fort Collins, Colo. (Chairman, Dept. of English and Modern Languages, Colorado State Univ., Fort Collins.)

7–12. Nuclear Physics, intern. cong., IUPAP, Paris, France. (C.I.P.N., Institut du Radium, II, rue Pierre Curie, Paris 5e.)

7–12. Paradontopathy Research, 15th Intern. cong., Paris, France. (Institut Universitaire de Medecine Dentaire, 24 Micheli-du-Crest, Geneva, Switzerland.)

11–13. Cardiovascular Surgery Cong., European Soc., Düsseldorf, Germany. (Congress Secretariat, Medizinische Akademie, Moorenstrasse 5, Düsseldorf.)

8–11. Institute of the Aeronautical Sciences, summer, Los Angeles, Calif. (S. P. Johnston, IAS, 2 E. 64 St., New York 21.)

9–15. Zoological Nomenclature Colloquium, London, England. (F. Hemming, 28 Park Village East, Regent's Park, London, N.W.1.)

10–14. Research Methods in Soil Zoology, colloquium, Harpenden, Hertfordshire, England. (P. W. Murphy, Rothamsted Experimental Station, Harpenden.)

12–14. Biological Sciences, intern. union, 13th general assembly, London, England. (Chairman, Div. of Biology and Agriculture, National Research Council, 2101 Constitution Ave., NW, Washington 25.)

(See issue of 16 May for comprehensive list)

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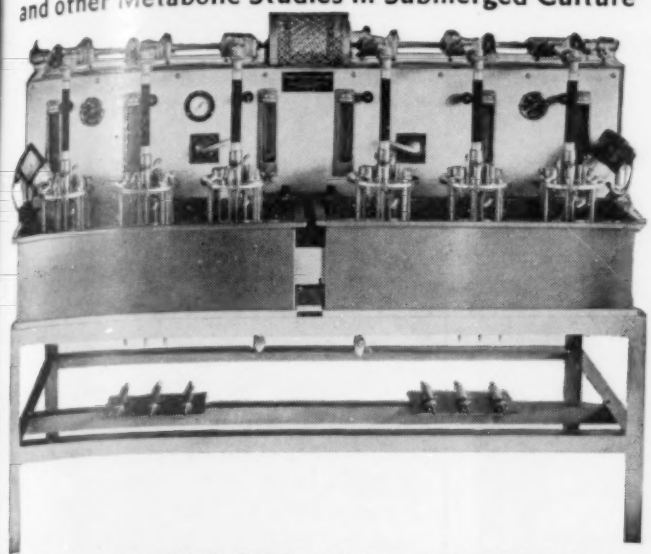

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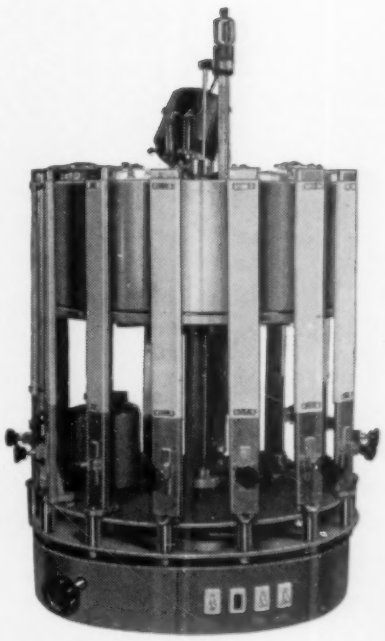

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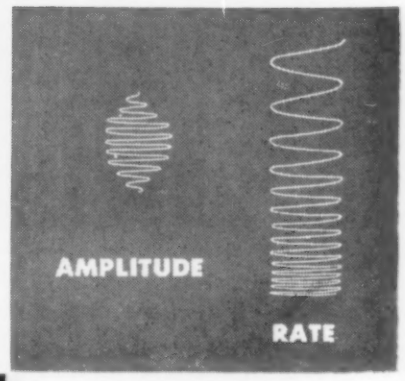

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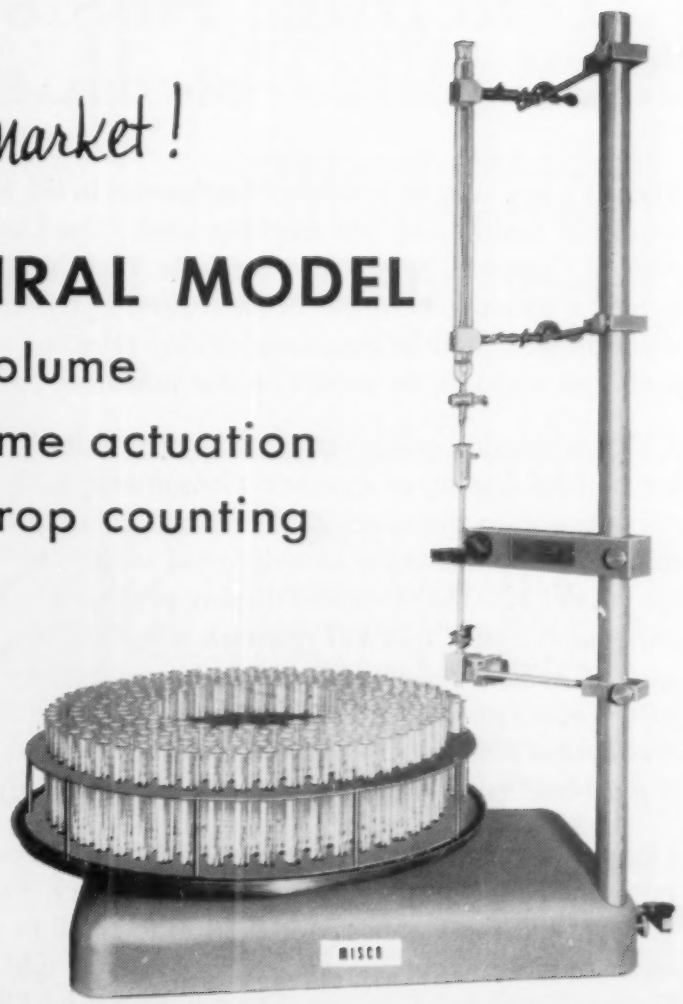

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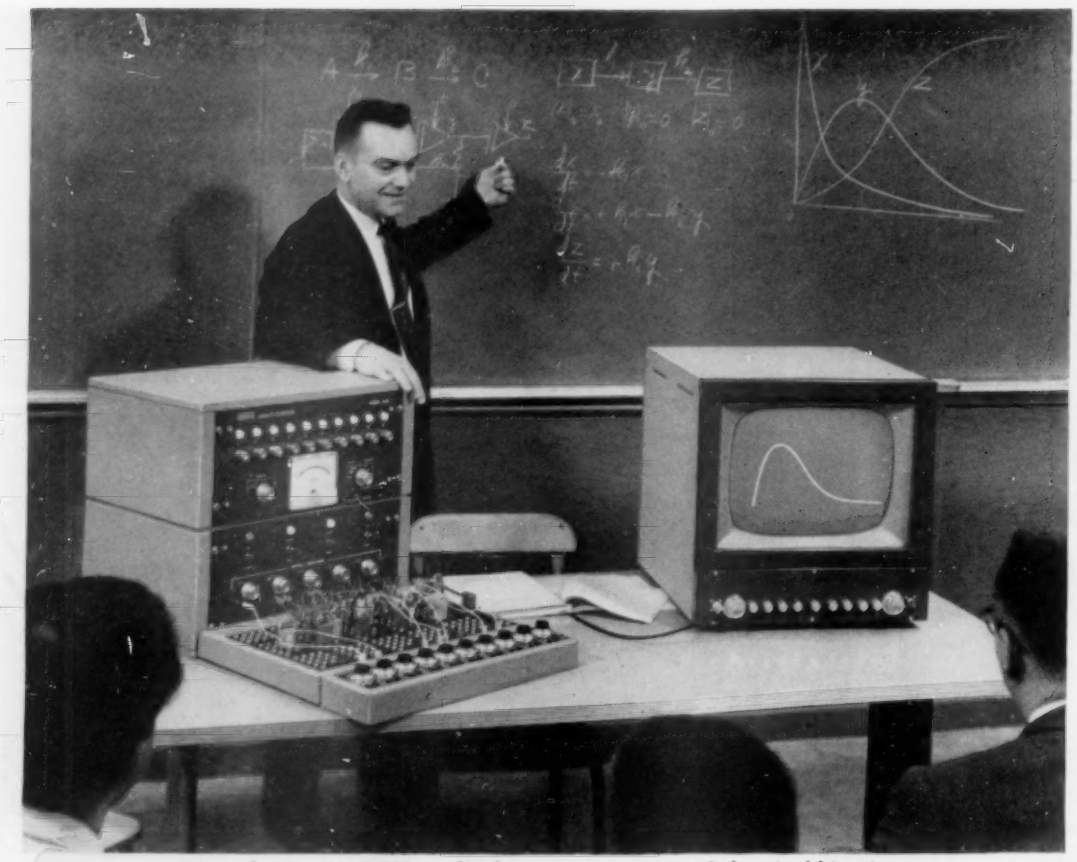

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Letters

The Responsibilities of Biologists

In *Science* of 7 February [127, 2[illegible] (1958)], Ileen E. Stewart presents ex[illegible] cerpts from an address with the abo[illegible] title given by the retiring president [illegible] the American Institute of Biological Sci[illegible] ences, H. Bentley Glass, at the Stanford[illegible] meetings last August. Lest readers of *Science* infer that biologists all approv[illegible] his rallying call, a response seems [illegible] order. At least some of us consider Glass's position confusing, if not confused.

"Responsibility" is predicated upon ethical or philosophical reference point[illegible] for which definition is essential. Spinoza would have been amused at Glass's equivocations, "the true welfare of hu[illegible] manity" and "social progress." No doubt Stalin stood for these also.

"Our first responsibility as biologists," says Glass, "is really to be biologists." But also, our "fundamental primary obli[illegible] gation" is "the stern duty to teach—to spread as widely as possible . . . com[illegible] prehension of the bases of a scientific civilization." The possibility of conflict between detachment and evangelism is ignored.

Concerning the "revolutionary and po[illegible] tentially devastating" powers arising from research, he says, "it is a frightening re[illegible] sponsibility . . . to see that these powers are used for good and not for harm." He deplores the fact that "so few biologists endeavor to make their biology count outside the laboratory and the class[illegible] room." He fears that what "we have gained through centuries of struggle . . . we may lose in a few months if we fail to defend stoutheartedly the freedom of the mind." He decries authority as "the inveterate foe of scientific inquiry." He worries about control of the purse strings of research and is pained that "the people and the representatives of the people feel that science is a useful servant or slave to minister to the needs of society as bidden." The distillation of such ideas may well be interpreted as a credo that scientists should run things since they should know what is "best." But, alas, Glass "would feel no confidence in asking the profession of biology to take over regulation of our government and our society," because of inexperience in politics. Considering the plethora of biological societies and how efficiently they operate, inexperience is an odd charge here; but at any rate, it appears that while condemning authority he really would not object to some sort of benevolent aristocracy or technocracy.

The plain fact is that scientists are only human, with the usual foibles and inconsistencies. We formulate the law of gravity but long to repeal it; the utility of death is abhorrent to us individually

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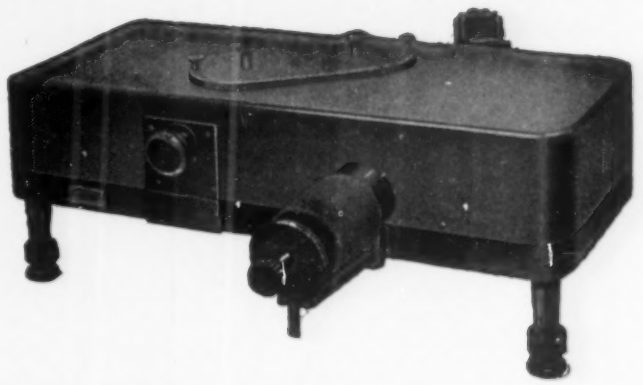

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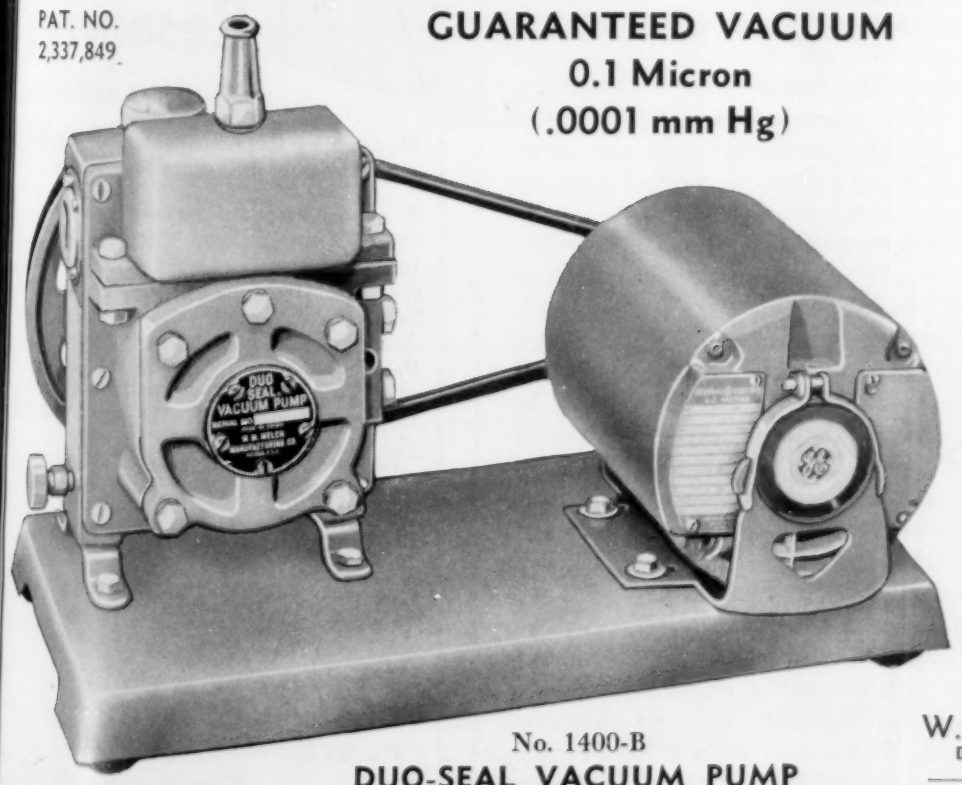

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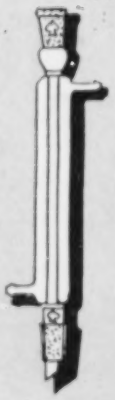

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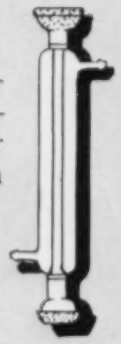

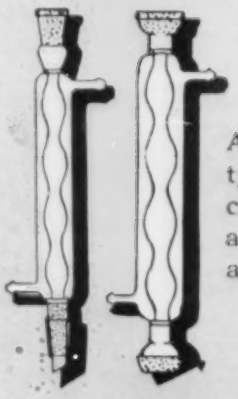

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and the regimentation elucidated in physiology is forgotten when we demand freedom. Biologists are shocked by polygamy, prefer fried chicken to raw, and calculate means for bimodal distributions. We teach evolution, if we can, but sing peace on earth and want our kids to be "normal." We know that symbiosis can lead to parasitism, yet we seek "grants" and favors.

How can Glass expect people so afflicted with professional schizophrenia to make our biology "count" outside the laboratory and classroom? Too often it "counts" very little even there. If we had the drive and acquisitiveness to practice our knowledge, we would soon be classed as tycoons or subversives, not placid members of the Society for Mycobryology in Pharmacological Pediculture.

Pragmatically our responsibilities are fixed by our agreements with others, notably those who provide us with support. As citizens we are bidden, as Glass puts it, to minister to the needs of society, but voluntary do-gooding is an assumption of responsibility not yet in the contract, and it can be our own undoing.

W. F. HOLLANDER
Department of Genetics,
Iowa State College, Ames

It is all too easy to string together a few quotations taken out of context and so convey an altogether misleading impression of superficiality and inconsistency. Personal correspondence from W. F. Hollander, much milder in tone than his communication to *Science,* indicates that he read not merely the excerpts of my address which were printed in *Science* but the full text, which was printed in the *AIBS Bulletin.* I am therefore all the more surprised that he makes so false an interpretation of my views, which, being a personal credo, I certainly do not expect every biologist to accept. I can reply within brief space to only a few of the erroneous conclusions Hollander has drawn.

The original text will show that I spoke of the biologist's responsibilities on three distinct levels: to himself, to his profession, and to society. It was in regard to the first that I said, "our first responsibility as biologists is really to be biologists," and the entire context will show that this carried no inference about detachment from society. My point was simply that biologists should exert themselves to maintain a working acquaintance with the entire scope of biology—should be biologists first and specialists second. There isn't the slightest conflict in that thought with what Hollander pleases to call "evangelism" and which I called teaching. This falls on the third level of responsibility. My point here is

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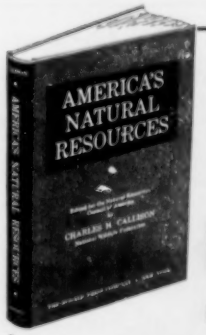

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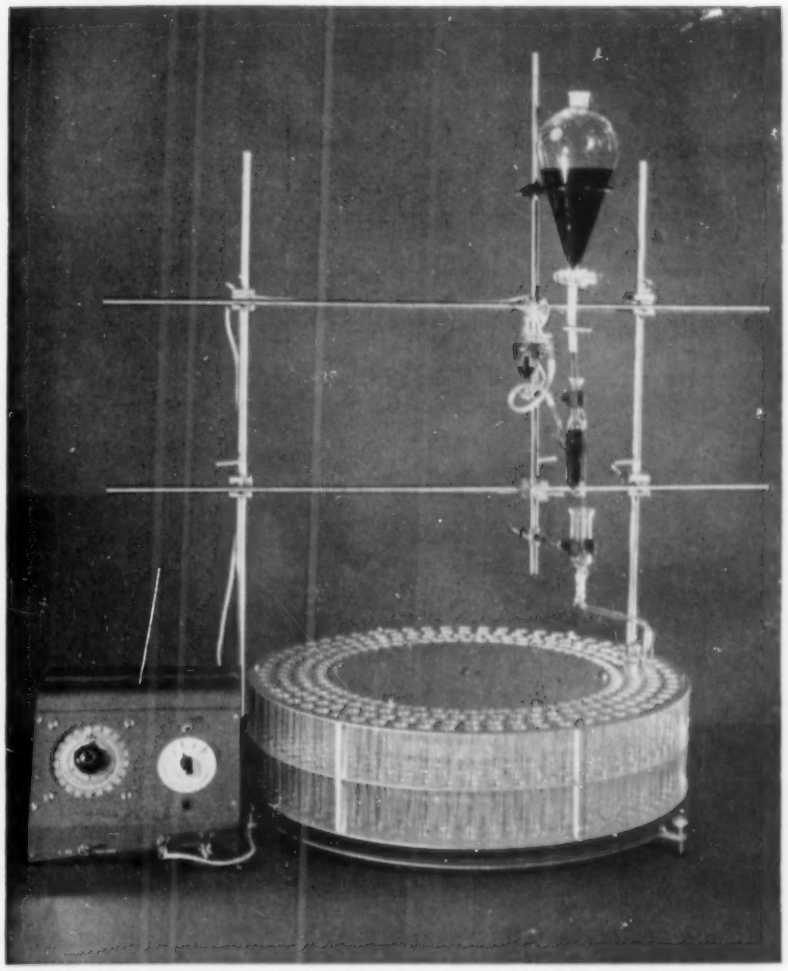

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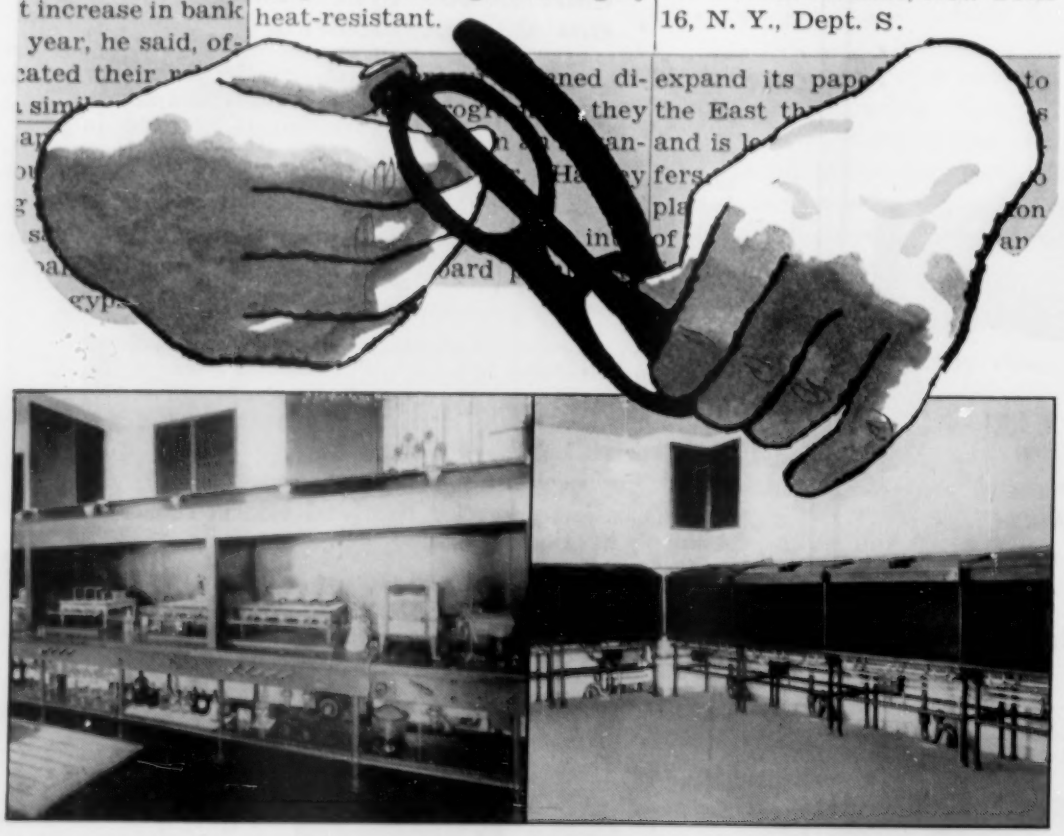

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a simple one: At the present tempo... social change and scientific advance, ... cannot confine ourselves to teaching ch... dren, and only children in the hope th... when, 20 years from now, they becom... adults they will act and vote more inte... ligently in matters affecting the relatio... of science to government and to socie... That will be very well, but it is imper... tive to attempt some education of prese... adults, including congressmen, gove... nors, and even Presidents, if an unde... standing of science adequate for th... framing of policy, especially in relati... to the governmental support of basic re... search, is to become developed in tim... The real difficulty is to distinguish be... tween education in scientific understand... ing and the promotion of political view... as the debate between Linus Pauling an... Edward Teller demonstrates. But how... ever much Hollander might want to ca... the *Bulletin of the Atomic Scientis...* "evangelism," I feel personally that ... has been one of the most significant an... influential developments of the postw... activities of scientists, and biologis... might well emulate that example.

Second, to say of me that "while con... demning authority he really would no... object to some sort of benevolent ari... tocracy or technocracy" reveals ver... careless reading of my remarks, since I was at pains to point out the danger o... oligarchy and tyranny if scientists were to continue to increase in power whil... the populace remained content to accep... the benefits of science in superstitiou... awe and credulity. Hollander's next para... graph, on the "plain fact . . . that scien... tists are only human, with the usual foibles and inconsistencies," is an elo... quent amplification of my own remark... to the same effect in the very address h... is criticizing—remarks which were made to emphasize, just as he does, that biolo... gists are hardly prepared to take over th... reins of government and society. Politica... inexperience is but one reason; but tha... it does exist seems evident from the fac... that, on the National Science Board, only three out of 24 appointees are biolo... gists, and that Killian's Scientific Ad... visory Committee to the President like... wise includes only three biologists out o... 19 members. The growing influence o... the American Institute of Biological Sci... ences is calculated gradually to brin... about a more appropriate representatio... of biologists on important advisor... boards. But lest I be misunderstood again, let me say emphatically that I d... not advocate "voluntary do-gooding," whatever that is, but rather the respon... sible service of biologists in more nu... merous advisory and educational capac... ities.

BENTLEY GLASS

Department of Biology,
Johns Hopkins University,
Baltimore, Maryland

Equipment

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Science does not assume responsibility for the accuracy of the information. A coupon for use in making inquiries concerning the items listed appears on page 1358.

FREQUENCY COUNTER is arranged to monitor any frequency in the range of 0 to 40,000 cy/sec. Upper and lower frequency limits can be set to define 1-cy frequency limits can be set to define 1-cy requirements. Deviation of the input signal beyond these limits actuates indicator lights and produces relay-contact closure for external control. Input frequency is also displayed. (Computer Measurements Corp., Dept. 71)

MICROPROJECTION APPARATUS attaches to any microscope with a monocular tube to project images on a French-lens viewing screen 5 in. in diameter. Focusing is accomplished by the microscope adjustments. The light source is not included. (William J. Hacker & Co., Inc., Dept. 93)

METABOLISM CAGES for tracer studies on small animals combines a glass cage with associated apparatus for control of air composition and for collection of the products of metabolism. Sizes suitable for rats and mice are available. (Delmar Scientific Laboratories, Dept. 94)

CHROMATOGRAPH speeds up paper chromatographic separations without affecting resolution. Centrifugal force is used to accelerate separation and is said to reduce the time required from hours or days to minutes. The equipment permits the flow rate to be held to less than 1 ml/min. Paper up to 35-cm in diameter is accommodated. Rotational speed is variable from 300 to 1550 rev/min. (Precision Scientific Co., Dept. 95)

MIXER-GRINDER is designed for preparation of laboratory-size samples. Treatment time is controlled by an automatic timer and is adjustable for periods up to 1 hour. Mixing is done in plastic vials by plastic balls. For grinding, case-hardened-steel or alumina-ceramic vials are available. The device is shock-mounted. (Spex Industries, Inc., Dept. 96)

INTEGRATOR for use with spectrophotometer converts the analog motion of the spectrophotometer recorder into digital pulses. The pulses are summed according to a program fed into the integrator by means of punched tape. The program may provide, in addition to sensitivity curves of the human eye, sensitivity of television receptors or color film, or data on light sources, to provide color matching for various viewers and various lighting conditions. Three sets of 200 or more reflectances per sample, cor-

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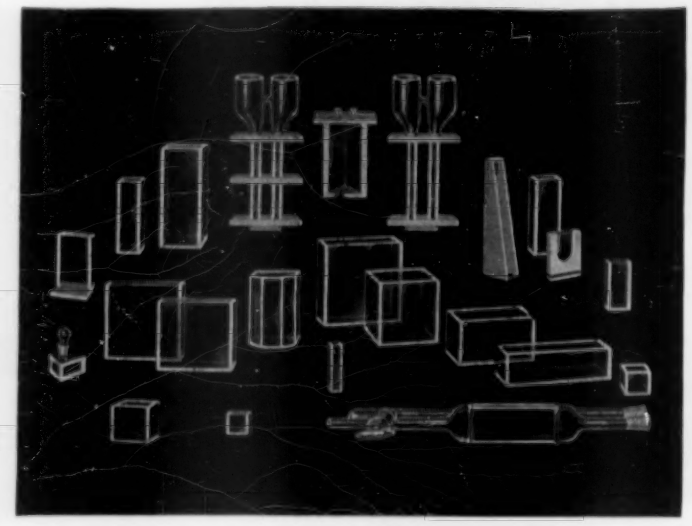

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...LSE-HEIGHT ANALYZERS, of 100 and ... channels, accumulate data for a ...et time after which the accumu...d record in its magnetic-core mem...is automatically printed out. A com...e print cycle is provided in 25 sec per ... channels. The analyzer memories ...be split into subgroups of 50 chan..., each externally programed. (Radia... Instrument Development Lab., Inc., ...pt. 92)

...ARMONIC GENERATOR provides a three...e signal composed of two sinusoidal ...ves and their composite for studying ...erference of waves. The composite is ...o heard through a speaker. Independ...t adjustments of amplitude and phase ...provided for each of the sinusoids. ...e instrument can also be used as a ...-speed (approximately 250 cy/sec) ...ree-channel electronic switch. (Madi... Associates, Inc., Dept. 98)

...EMPERATURE TEST CHAMBERS feature ...-temperature ranges of −40°, −100°, ...d −120°F and high-temperature ...ges of 240° or 350°F. Relative hu...dities from 20 to 90 percent can also ... provided. Working space is 30 ft³, ...d outer dimensions are 4½ by 5 by ...¼ ft. Installation requires only plug in ... electrical connection. (Tenney Engi...ering Inc., Dept. 105)

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■ WIDE-RANGE TEST RECEIVER covers the frequency range from 30 Mcy/sec to 75 kMcy/sec without plug-in units. Linear dynamic range exceeds 40 db. Sensitivity is greater than −90 dbm at 30 Mcy/sec and −70 dbm at 75 kMcy/sec. No waveguide connections from receiver to antenna are required. (Scientific-Atlanta Inc., Dept. 103)

■ INFRARED DETECTION DEVICE is available in either focused-beam or wide-angle versions. Radiation at the 10^{-9}w level is said to be detectable. The detector is transistorized and will operate for several months from the self-contained power source. (Lindly & Co., Dept. 101)

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National Bureau of Standards

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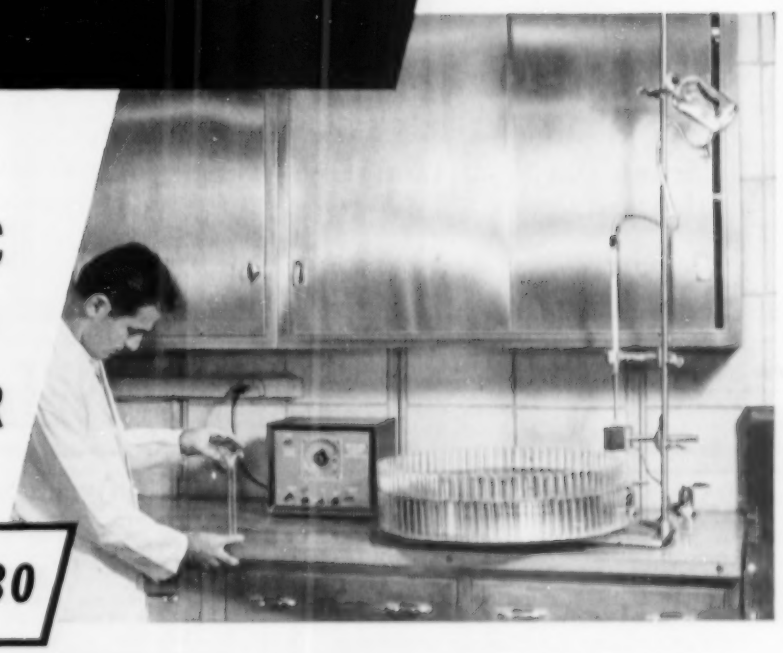

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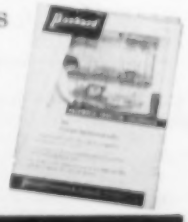

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